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A

SYSTEM

OF

MECHANICAL PHILOSOPHY.

BY JOHN ROBISON, LL. D.

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UNIVERSITY OF EDINBURGH.

WITH NOTES,

BY DAVID BREWSTER, LL. D.

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ROYAL SOCIETY OF EDINBURGH.

IN FOUR VOLUMES,

AND A VOLUME OF PLATES.

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CONTENTS

OF

VOLUME THIRD.

Page

Astronomy — 1

Telescope — 403

Pneumatics — 523

QC21
R56
v.3

ASTRONOMY.

1. **Astronomy** was first studied as an art, subservient to the purposes of social life. Some knowledge of the celestial motions was necessary, in every state of society, that we might mark the progress of the seasons, which regulate the labours of the cultivator, and the migrations of the shepherd. It is necessary for the record of past events, and for the appointment of national meetings.

While the motions of the heavenly bodies afford us the means of attaining these useful ends, they also present to the curious philosopher a series of magnificent phenomena, the operation of the greatest powers of material nature; and thus they powerfully excite his curiosity with respect to their causes. This circumstance alone makes the celestial motions the proper objects of attention to a student of Mechanical Philosophy, and he has less concern in the beautiful regularity and subordination which have made them so subservient to the purposes of Navigation, of Chronology, and the occupations of rural life.

But the purposes of the mechanical philosopher cannot be attained without attending to that beauty, regularity, and subordination. These features are exhibited in every circumstance of the celestial motions that renders them susceptible of scientific arrangement and investigation; and a philosophical view cannot be taken, without the same accurate knowledge of the motions that is wanted for the arts of life. It must be added, that society never would have derived the benefits which it has received from astronomy, without the labours of the philosopher; for, had not Newton, or some such exalted genius as Newton, speculated about the deflecting forces which regulate the motions of the solar system, we never should have acquired that exquisite knowledge of the mere phenomena that is absolutely necessary for some of the most important applications of them to the arts. It was these speculations alone that have enabled our navigators to proceed with boldness through untried seas, and in a few years have almost completed the survey of this globe. And thus do we experience the most beneficial alliance of philosophy and art.

Since the motions of bodies are the only indications, characteristics, and measures of moving forces, it is plain, that the celestial motions must be accurately ascertained, that we may obtain the data wanted for the purpose of philosophical inference. To ascertain these is a task of great difficulty; and it has required the continual efforts of many ages to acquire just notions of the motions exhibited to our view in the heavens. For the same general appearances may be exhibited, and the same perceptions obtained, and the same opinions will be formed, by means of motions very different; and it is frequently very difficult to select those motions which alone can exhibit *every* observed appearance. If a person who is in motion imagines that he is at rest, and assumes this principle in his reasonings about the effects of the motions which he perceives, he mistakes the conclusions which he draws for real perceptions; and calls that a deception of sense, which is really an error in judgment. Errors, in our opinions concerning the motions of the heavenly bodies, are necessarily accompanied by false judgments concerning their causes. Therefore, an accurate examination of the motions which really obtain in the heavens, must precede every attempt to investigate their causes.

The most probable plan for acquiring a just and satisfactory knowledge of these particulars, is to follow the steps of our predecessors in this study, and first to consider the more general and obvious phenomena. From these we must deduce the opinions which most obviously suggest themselves, to be corrected afterwards, by comparing them with other phenomena, which may happen to be irreconcilable with them.

ASTRONOMICAL PHENOMENA.

2. To an observer, whose view on all sides is bounded only by the sea, the heavens appear a concave sphere, of which the eye is the centre, studded with a great number of luminous bodies, of which the Sun and Moon are the most remarkable. This sphere is called the SPHERE OF THE STARRY HEAVENS.

The only distances in the heavens which are the immediate objects of our observation, are arches of great circles passing through the different points of the starry heavens. Therefore, all astronomical computations and measurements are performed by the rules of spherical trigonometry.

3. We see only the half of the heavens at a time, the other half being hid by the earth on which we are placed. The great circle H B O D, (Fig. 1.) which separates the visible hemisphere H Z O from the invisible hemisphere H N O, is called the HORIZON. This is marked out on the starry heavens by the farthest edge of the sea. The point Z immediately over the head of the observer is called the ZENITH; and the point N, diametrically opposite to it, is called the NADIR.

4. The zenith and nadir are poles of the horizon.

5. If an observer looks at the heavens, while a plummet is suspended before his eye, the plumb line will mark out on the heavens a quadrant of a circle, whose plane is per-

pendicular to the horizon, and which therefore passes through the zenith and nadir, and through two opposite points of the horizon. Z O N H and Z B N D are such circles. They are called VERTICAL CIRCLES and AZIMUTH CIRCLES.

6. The ALTITUDE of any celestial phenomenon, such as a star A, is the angle A C B, formed in the plane of the vertical circle Z A N, by the horizontal line C B and the line C A. This name is also given to the arch A B of the vertical circle which measures this angle. The arch Z A is called ZENITH DISTANCE of the phenomenon.

7. The AZIMUTH of the phenomenon is the angle OCB, or O Z B, formed between the plane of the vertical circle Z A B passing through the phenomenon, and the plane of some other noted vertical Z O N. The arch C B of the horizon, which measures this angle, is also frequently called the azimuth.

8. The starry heavens appear to turn round the earth, which seems pendulous in the centre of the sphere; and by this motion, the heavenly bodies come into view in the east, or RISE; they attain the greatest altitude, or CULMINATE, and disappear in the west, or SET. This is called the FIRST MOTION.

9. This motion is performed round an axis N S (Fig. 2.) passing through two points, N, S, called the poles of the world. In consequence of this motion, a celestial object A describes a circle AD B F, through the centre C of which the axis N S passes, perpendicularly to its plane. This motion may be very distinctly perceived as follows: Let a point, or sight, be fixed in the inside of a sky-light fronting the north, and inclined southwards from the perpendicular at an angle equal to the latitude of the place. An eye placed at this point will see the stars through the glass of the window. Let the points of the glass, through which a star appears from time to time, be marked. The marks will be found to lie in the circumference of a circle,

the centre of which will mark the place of the pole in the heavens.

10. Those stars which are farthest from the poles will describe the greatest circles; and those will describe the largest possible circles which are in the circumference of the circle ÆW Q E, which is equidistant from both poles. This circle is called the EQUATOR, and, being a great circle, it cuts the horizon in two points, E, W, diametrically opposite to each other. They are the east and west points of the horizon.

11. If a great circle A N Q S Æ passes through the poles perpendicularly to the horizon H W O E, it will cut it in the north and south points; and any star A will acquire its greatest elevation when it comes to the semicircle N A S, and its greatest depression when it comes to the semicircle N B S; and the arch D A F of its apparition will be bisected in A.

12. If the circle A D B F of revolution be between the equator and that pole N which is above the horizon, the greatest portion of it will be visible; but if it be on the other side of the equator, the smallest portion will be visible. One half of the equator is visible. Some circles of revolution are wholly above the horizon, and some are wholly below it. A star in one of the first is always seen, and one in the last is never seen.

13. The distance A Æ of any point A from the equator is called its DECLINATION, and the circle A D B F, being parallel to the equator, is called a PARALLEL OF DECLINATION.

14. The angle Æ C H, contained by the planes of the equator and horizon, is the complement of the angle N C O, which is the elevation of the pole.

15. The revolution of the starry heavens is performed in $23^h\ 56'\ 4''$. It is called the DIURNAL REVOLUTION. No appearance of inequality has been observed in it;

and it is therefore assumed as the most perfect measure of time.

16. The time of the diurnal apparition or disparition of a point of the starry heavens is bisected in the instant of its culmination or greatest depression. The Sun, therefore, is in the circle N A S Q at noon. For this reason the circle N A S Q is called the MERIDIAN.

17. A phenomenon whose circle of diurnal revolution A D B F is on the same side of the equator with the elevated pole, is longer visible than it is invisible. The contrary obtains if it be on the other side of the equator.

18. Any great circle N A Æ S, or N B L S, (Fig. 3.) passing through the poles of the world, is called an HOUR CIRCLE.

19. The angle Æ C L, or Æ N L, contained between the plane of the hour-circle N B L S, passing through any phenomenon B, and the plane of the hour circle N Æ S, passing through a certain noted point Æ of the equator, is called the RIGHT ASCENSION of the phenomenon. The intercepted arch Æ L of the equator, which measures this angle, is called by the same name.

20. In assigning the place of any celestial phenomenon, we cannot use any points of the earth as points of reference. The starry heavens afford a very convenient means for this purpose. Most of the stars retain their relative situations, and may therefore be used as so many points of reference. The application of this to our purpose requires a knowledge of the positions of the stars. This may be acquired. The difference between the meridional altitude of a star B, and of the equator, gives the arch A Æ, intercepted between the equator and the parallel of declination, or circle of diurnal revolution A B D, described by the star. And the time which elapses between the passage of this star over the meridian, and the passage of that point Æ of the equator from which the right of ascensions are computed, gives the arch Æ L of the equator which has

passed during this interval. Therefore, an hour circle N L S being drawn through the point L of the equator, and a circle of revolution A B D being drawn at the observed distance A Æ from the equator, the place of the star will be found in their intersection B.

21. Globes and maps have been made, on which the representations of the stars have been placed, in positions similar to their real positions; and catalogues of the stars have been composed, in which every star is set down with its declination and right ascension, this being the most convenient arrangement for the practical astronomer. Their longitudes and latitudes (to be explained afterwards) are also set down, in separate columns. The most noted of all these is the BRITANNIC CATALOGUE, constructed by Dr Flamstead, from his own observations in the Royal Observatory at Greenwich. This catalogue contains the places of 3000 stars. It is accompanied by a collection of maps, known to all astronomers by the title of ATLAS CELESTIS. An useful abridgment of both has been published by *Bode* in *Berlin*, and by *Fortin* in *Paris*, in small quarto. Two planispheres have also been published by *Senex*, in *London*, constructed from the same observations, and executed with uncommon elegance; as also a particular map of that zone of the heavens to which all the planetary motions are limited. This is also executed with superior elegance and accuracy. The place of any phenomenon may be ascertained in it within 5′ of the truth, by mere inspection, without calculation, scale, or compasses. No astronomer should be unprovided with it.

22. All these representations and descriptions of the starry heavens become obsolete, in some measure, in consequence of a gradual change in the declination and right ascension of the stars. But as this may be accurately computed, the maps and catalogues retain their original value, requiring only a little trouble in accommodating them to the present state of the heavens. The Britannic

Catalogue and Atlas are adjusted to the state of the heavens in 1690; and the planispheres, &c. by Senex, are the same. The editions of Paris and Berlin are for 1750.

23. In these maps and catalogues, it has been found convenient to distribute the stars into groupes, called CONSTELLATIONS; and figures are drawn, which comprehend all the stars of a group, and give them a sort of connexion and a name. Each star is distinguished by its number in the constellation, and also by a letter of the alphabet. Thus, the most brilliant star in the heavens, the Dog star, or Sirius, is known to all astronomers as No 9, or as α *Canis Majoris*. The numbers always refer to the Britannic catalogue, it being considered as classical.

24. Since the publication of that work, however, great additions have been made to our knowledge of the starry heavens, and several catalogues and atlases have been published in different parts of Europe. Of the catalogues, the most esteeemed are, 1. a small catalogue of 389 stars, the places of which have been determined with the utmost care by Dr Bradley, at the Greenwich Observatory; 2. a catalogue of the southern stars by Abbé de la Caille; 3. a catalogue of the zodiacal stars by Tobias Mayer at Gottingen; and, *lastly*, a new atlas celestis, consisting of a catalogue and maps of the whole heavens, and containing above 15,000 stars, by Mr Bode of Berlin. The Rev. Mr Fr. Wollaston published, in 1780, a specimen of a general astronomical catalogue of the fixed stars, arranged according to their declinations, folio, London, 1780. This is a most valuable work, containing the places of many thousand stars, according to the catalogues of Flamstead, La Caille, Bradley, and Mayer. These being arranged in parallel columns, we see the differences between the determinations of those astronomers, and are advertised of any changes which have occurred in the heavens. The catalogue is accompanied by directions for prosecuting this

method of obtaining a minute survey of the whole starry heavens.

In the valuable astronomical tables published in 1776 by the academy of Berlin, Mr Bode has given a similar synopsis of the catalogues of Flamstead, La Caille, Bradley, and Mayer, not indeed so extensive, nor so minute, as Wollaston's, but of great use.[*]

25. Having thus obtained maps of the heavens, the place of a celestial phenomenon is ascertained in a variety of ways. 1. By its observed distance from two known stars. 2. By its altitude and azimuth. 3. Most accurately, by its right ascension and declination.

26. This last being the most accurate method of ascertaining the place of any celestial phenomenon, observations of meridional altitude, and of TRANSITS over the meridian, are the most important. For an account of the manner of conducting these observations, and a description of the instruments, we may consult Smith's Optics, vol. II.; Mr Vince's Treatise of Practical Astronomy; La Lande's Astronomy, &c.[†] The MURAL QUADRANT, TRANSIT INSTRUMENT, and CLOCK, are therefore the capital furniture of an observatory; to which, however, should be added an EQUATORIAL INSTRUMENT for observing phenomena out of the meridian. Other instruments, such as the EQUAL ALTITUDE INSTRUMENT, the RHOMBOIDAL RETICULA, the ZENITH SECTOR, and one or two more, are fitted for astronomers on a voyage.

27. The position of the meridian, and the latitude of the observatory, must be accurately determined. There are va-

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[*] This Catalogue, reduced to 1820, is published in the EDINBURGH ENCYCLOPÆDIA, Art. ASTRONOMY, vol. II. p. 745.——ED.

[†] An account of the modern astronomical instruments, as made by Mr Troughton, and the method of using them, will be found in the EDINBURGH ENCYCLOPÆDIA, Articles ASTRONOMY, CIRCLE, OBSERVATORY, &c.——ED.

rious methods of determining the meridian. The most accurate is to view a circumpolar star through a telescope which has an accurate motion in a vertical plane, and to change the position of the telescope till the times which elapse between the successive upper and lower transits of the star are precisely equal. The instrument is then in the plane of the meridian (Fig. 4.)

28. In order to find the declination of a phenomenon more readily, it is convenient to know the inclination of the axis of diurnal revolution N S (Fig. 2.) to the horizon, or the elevation of the pole N. The best method for this purpose is to observe the greatest elevation I O, and the least elevation K O, of some circumpolar star. The elevation of the pole N is half the sum of those elevations.

29. The elevation of the pole is different in different places. An observer, situated 69½ statute miles due north of another, will find the pole elevated about a degree more above his horizon. From observations of this kind, the bulk and shape of the earth are determined. For it is plain that 360 times 69½ miles must be the circumference of the globe. It is found to be nearly an elliptical spheroid, of which the axis is 7904 miles, and the greatest diameter 7940 miles. This deviation from perfect sphericity has been discovered by measuring, in the way now mentioned, a degree of the meridian in different latitudes. One was measured in Lapland, in latitude 66° 20′, and it measured 122,457 yards, exceeding 69½ miles by 137 yards*. Another was measured at Peru, crossing the very equator. It contained 121,027 yards falling short of 69½ miles by 1293 yards, and wanting 1430 yards, or almost a mile, of the other. Other degrees have been measured in intermediate latitudes; and it is clearly established, that the degrees gra-

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* This degree is supposed to be 200 French toises too great by M. Svanberg, who lately remeasured it.—Ed.

dually increase, as we go from the equator towards either pole.

30. The length of a degree is the distance between two places where the tangents to the surface are inclined to one another one degree, or where two plumb lines, which are perpendicular to the surface of standing water, will, when produced downwards, meet one another, intercepting an angle of one degree. The surface of the still ocean is therefore less incurvated as we approach the poles, or it requires a longer arch to have the same curvature. It is a degree of a larger circle, and has a longer radius. Persons who do not consider the thing attentively, are apt to imagine, from this, that the earth is shaped like an egg; because, if we draw from its centre lines C N (Fig. 5.) C O, C P, C Q, equally inclined to one another, the arches N O, O P, P Q, will gradually increase from N towards Q. If these lines make angles of one degree with one another, they will meet the surface in points that are farther and farther asunder, and the degree will appear to increase as we approach the points E and Q, which we suppose, at present, to be the poles. But let such persons reflect, that if these lines from the centre are produced beyond the surface, they cannot be plumb lines, perpendicular to the surface of standing water. But if an ellipse N E S Q (Fig. 5.) be made to turn round its shorter axis N S, it will generate a figure flatter round N and S than at E or Q. If we draw two lines *a* D and *b* B perpendicular to the curve in *a* and *b*, and exceedingly near one another, they will be tangents to a curve A B D F, by the evolution of which the elliptic quadrant E *a* N is described. A E is the radius of curvature of the equatoreal degree of the meridian E *a* N. N F is the radius of the polar degree, and *a* D is the radius of curvature at the intermediate latitude of *a*, &c. All these radii are plumb lines, perpendicular to the elliptical curve of the ocean.

These plumb lines, therefore, do not meet in the centre of the earth, as is commonly imagined, but meet in succession, in the circumference of the evolute A B D F. The earth is not a *prolate* spheroid like an egg, but an *oblate* spheroid, like a turnip or bias bowl.

31. Since the axis of diurnal revolution passes through the centre of the earth, it marks on its surface two points, which are the poles of the earth. These are in the extremities of the axis of the terrestrial spheroid. In like manner, the plane of the celestial equator, passing through the centre of the earth, divides it into two hemispheres, the northern and southern, separated by the *terrestrial* equator. Also, the hour circles, passing through the earth's centre, mark on its surface the terrestrial meridians.

32. The position of a place on the surface of the earth is determined by its LATITUDE, or distance from the terrestrial equator, and its LONGITUDE, or the angular distance of its meridian, from some noted meridian.

33. Astronomical observations are made from a point on the surface of the earth, but, for the purposes of computation, are supposed to be made from the centre. The angular distance between the observed place A (Fig. 6.) of a phenomenon S in the heavens, as seen from a place D on the earth's surface, and its place B, as viewed from the centre, is called the PARALLAX of the phenomenon.

34. Besides the motion of diurnal revolution, common to all the heavenly bodies, there are other motions, which are peculiar to some of them, and are observed by us by means of their change of place in the starry heavens. Thus, while the starry heavens turn round the Earth from east to west in $23^h\ 56'\ 4''$, the Sun turns round it in 24^h. He *must*, therefore, change his place to the eastward in the starry heavens. The Moon has an evident motion eastward among the stars, moving her own breadth in about an hour. There are five stars which are observed to change their places remarkably in the heavens, and are therefore called PLANETS, or wanderers; while those which

do not change their relative places are called FIXED STARS. The planets are, Mercury, Venus, Mars, Jupiter, and Saturn. To these we must now add the planet discovered in 1781 by Dr Herschel, which he called the Georgian Planet, in honour of his sovereign George III. the distinguished patron of astronomy. Astronomers on the continent have not adopted this denomination, and seem generally agreed to call it by the name of the discoverer. M. Piazzi, at Palermo, has discovered another, and M. Olbers, at Bremen, a third, which they have named Ceres and Pallas.* None of the three are visible to the naked eye.

34. Planets are distinguishable from the fixed stars by the steadiness of their light, while all the fixed stars are observed to twinkle. The following symbols are frequently used:

For the Sun	☉	For Mars	♂
the Moon	☽	Jupiter	♃
Mercury	☿	Saturn	♄
Venus	♀	Herschel or Uranus	H.
the Earth	♁		

The motions of these bodies have become interesting on various accounts. In order to acquire a knowledge of their motions more easily, it is convenient to abstract our attention from the diurnal motion, common to all, and attend only to their proper motions among the fixed stars.

Of the proper Motions of the Sun.

35. We cannot observe the motion of the Sun among the fixed stars immediately, on account of his great splendour, which hinders us from perceiving the stars in his

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* A fourth, called *Juno*, was afterwards discovered by M. Harding in 1804, and a fifth, called *Vesta*, by Dr Olbers, in 1807. A very full account of the Four New Planets will be found in the Supplement to Ferguson's Astronomy.—Ed.

neighbourhood. But we can observe the instant of his coming to the meridian, and his meridional altitude (20.) The Sun must be in that point of the heavens which passes the meridian at that instant, and with that altitude. Or we can observe the point of the heavens which comes to the meridian at midnight, with a declination as far on one side of the equator as the Sun's observed declination is on the other side of it. The Sun must be in the point of the heavens which is diametrically opposite to this point. By taking either of these methods, but particularly the first, we can ascertain a series of points of the heavens through which the Sun passes. These are found to be in the circumference of a great circle of the sphere A S V W (Fig. 7.), which cuts the celestial equator in two opposite points, A, V, and is inclined to it at an angle of 23° 28′ nearly. This circle, or Sun's path, is called the ECLIPTIC.

36. In consequence of the obliquity of the ecliptic, the Sun's motion in it is accompanied by a change in the Sun's declination and right ascension, by a change in the length of the natural day, and by a change of the seasons. Therefore, the revolution of the Sun in the ecliptic is performed in a year.

37. The points, V, A, are called EQUINOCTIAL POINTS; because, when the sun is in these points, his circle of diurnal revolution is the celestial equator, and therefore the day and night are equal. The point V, through which he passes in the month of March, is called the VERNAL EQUINOX, and the point A is called the AUTUMNAL EQUINOX. The points S and W, where he is farthest from the equator, are called the SOLSTITIAL POINTS, S being the summer, and W the winter solstice. The parallels of declination passing through the solstitial points are called TROPICS.

38. Right ascension is always computed eastward on the equator, from the vernal equinox.

39. The ecliptic passes through the constellations

Aries, distinguished by the symbol	♈	Libra, distinguished by the symbol	♎
Taurus	♉	Scorpio	♏
Gemini	♊	Sagittarius	♐
Cancer	♋	Capricornus	♑
Leo	♌	Aquarius	♒
Virgo	♍	Pisces	♓

These constellations are called the SIGNS of the ZODIAC; and a motion from west to east is said to be DIRECT, or IN CONSEQUENTIA SIGNORUM, while a contrary motion is called RETROGRADE, IN ANTECEDENTIA SIGNORUM.

40. The changes of the seasons were attributed by the ancients to the influence of the stars which were seen in the different seasons of the year.

41. The position of the ecliptic is invariable, and a complete revolution is performed in 365 days, 6 hours, 9 minutes, and 11 seconds.

42. If successive observations be made of the Sun's crossing the equator, it will be found that the equinoctial points are not fixed, but move to the westward about 50″ in a year, so that they would make a complete revolution in about 25,972 years. This is called the PROCESSION of the EQUINOXES.

43. Sir Isaac Newton made a very ingenious and important inference from this astronomical fact. If we know the situation of the equinoctial points at the time of any historical event, the date of the event may be discovered. He thinks that this position, at the time of the Argonautic expedition, may be inferred from the description given by Aratus of the starry heavens. The poet describes a celestial sphere by which Chiron, one of the heroes, directed their motions; and from this he deduces data for a chronology of the heroic or fabulous ages. But, since the equinoctial points shift only at the rate of a degree in 72 years, and the Greeks were so ignorant, for ages after that epoch,

that they did not know that the positions of the stars were changeable, it does not appear that much reliance can be had on this datum. We cannot, from the description by Aratus, be certain of the position of the vernal equinox within five or six degrees. This makes a difference of 400 years in the epochs.

44. The axis of diurnal revolution is not always the same, and the poles of the heavens describe (in 25,972 years) a circle round the pole of the ecliptic, distant from it 23° 28′ 10″ nearly.

45. On account of the westerly motion of the equinoctial points, the return of the seasons must be accomplished in less time than that of the Sun's revolution round the heavens. The seasons return after an interval of $365^d\ 5^h\ 48'\ 45''$. This is called a TROPICAL year, to distinguish it from the interval $365^d\ 6^h\ 9'\ 11''$ called a SYDEREAL year.

46. Astronomers have chosen to refer the places of the heavenly bodies to the ecliptic, on account of its stability, rather than to the equator. For this purpose, great circles, such as PVp, PAp (Fig. 8.) are drawn through the poles P, p, of the ecliptic. These are called ECLIPTIC MERIDIANS. The arch AB of one of these circles, intercepted between a phenomenon A and the ecliptic, is called the LATITUDE of the phenomenon; and the arch VB, intercepted between the point V of the vernal equinox and the point B, is called the LONGITUDE of the phenomenon. This is sometimes expressed in degrees and minutes, and sometimes in signs, degrees, and minutes, each sign consisting of thirty degrees.

47. The motion of the Sun in the ecliptic is not uniform. On the first of January his daily motion is nearly 1° 1′ 13″. But on the first of July, his daily motion is 57′ 13″. The mean daily motion is 59′ 08″. The Sun's place in the ecliptic, calculated on the supposition of a daily motion of 59′ 08″, will be behind his observed place, from the beginning of January to the beginning of July, and will be before it, from the beginning of July to the beginning of

January. The greatest difference is about 1° 55′ 32″, which is observed about the beginning of April and October; at which times, the observed daily motion is 59′ 08″.

48. This unequable motion of the Sun appeared to the ancient astronomers to require some explanation. It had been received as a first principle, that the celestial motions were of the most perfect kind—and this perfection was thought to require invariable sameness. Therefore the Sun must be carried uniformly in the circumference of a figure perfectly uniform in every part. He must therefore move uniformly in the circumference of a circle. The astronomers therefore supposed that the earth is not in the centre of this circle. Let A *b* P *d* (Fig. 9.) represent the Sun's orbit, having the earth in E, at some distance from the centre C. It is plain, that if the Sun's motion be uniform in the circumference, describing every day 59′ 08″, his angular motion, as seen from the earth, must be slower when he is at A, his greatest distance, than when nearest to the earth, at P. It is also evident that the point E may be so chosen, that an arch of 59′ 08″ at A shall subtend an angle at E that is only 57′ 13″, and that an arch of 59′ 08″ at P shall subtend an angle of 61′ 13″. This will be accomplished, if we make E P to E A as 57′ 13″ to 61′ 13″, or nearly as 14 to 15. This was accordingly done; and this method of solving the appearances was called the *eccentric hypothesis*. E C is the ECCENTRICITY, and P E is to P C nearly as 28 to 29.

49. But although this hypothesis agreed very well with observation in those points of the orbit where the Sun is most remote from the earth, or nearest to it, it was found to differ greatly in other parts of the orbit, and particularly about half way between A and P. Astronomers, after trying various other hypotheses, were obliged to content themselves with reducing the eccentricity considerably, and also to suppose that the angular motion of 59′ 08″ per day was performed round a point *e* on the other side of the

centre, at the same distance with E. This, however, was giving up the principle of perfect motion, if its perfection consisted in uniformity; for, in this case, the Sun cannot have an uniform motion in the circumference, and also an uniform angular motion round *e*. Besides, even this amendment of the eccentric hypothesis by no means agreed with the observations in the months of April and October; but they could not make it any better.

50. Astronomical computations are made on the supposition of uniform angular motion. The angle proportional to the time is called the MEAN MOTION, and the place thus computed is called the MEAN PLACE. The differences between the mean places and the observed, or TRUE PLACES, are called EQUATIONS. They are always greatest when the mean and true motions are equal, and they are nothing when the mean and true motions differ most. For, while the true daily angular motion is less than the mean daily motion, the observed place falls more and more behind the calculated place every day; and although, by gradually quickening, it loses less every day, it still loses, and falls still more behind; and when the true daily motion has at last become equal to the mean, it loses no more indeed, but it is now the farthest behind that can be. Next day it gains a little of the lost ground, but is still behind. Gaining more and more every day, by its increase of angular motion, it at last comes up with the calculated place; but now, its angular motion is the greatest possible, and differs most from the equable mean motion.

51. These computations are begun from that point of the orbit where the motion is slowest, and the mean angular distance from this point is called the MEAN ANOMALY. A table is made of the equations corresponding to each degree of the mean anomaly. The true anomaly is found by adding to, or subtracting from the computed mean anomaly, the equations corresponding to it.

In this manner may the Sun's longitude, or place in the ecliptic, be found for any time.

52. In consequence of the obliquity of the ecliptic, and the Sun's unequal motion in it, the natural days, or the interval between two successive passages of the Sun over the meridian, are unequal; and if a clock, which measures $365_d\ 5^h\ 48'\ 50''$ in a tropical year, be compared from day to day with an exact sun dial, they will be found to differ, and will agree only four times in the year. This difference is called the EQUATION of TIME, and sometimes amounts to 16 minutes. The time shewn by the clock is called MEAN SOLAR TIME, and that shewn by the dial is called TRUE TIME and APPARENT TIME.

53. The change in the Sun's motion is accompanied by a change in his apparent diameter, which, at the beginning of January, is about $32'\ 39''$, and at the beginning of July is about $31'\ 34''$, $\frac{1}{30}$ less. This must be ascribed to a change of distance, which must always be supposed inversely proportional to the apparent diameter.

54. By combining the observations of the sun's place in the ecliptic with those of his distance, inferred from the apparent diameter, and by other more decisive, but less obvious observations, Kepler, a German astronomer, found that his apparent path round the earth is an ellipse, having the earth in one focus, and having the longer axis to the shorter axis as 200,000 to 199,972.

The extremities A and P of the longer axis of the sun's orbit A B P D (Fig. 9) are called the APSIDES. The point A, where the sun is farthest from the earth (placed in E), is called the higher apsis, or APOGEE. P is the lower apsis, or PERIGEE. The distance E C between the focus and centre is called the ECCENTRICITY, and is 1680 parts of a scale, of which the mean distance E D is 100,000.

55. Kepler *observed*, that the sun's angular motion in this orbit was inversely proportional to the square of his distance from the earth; for he observed the sun's daily

change of place to be as the square of his apparent diameter. Hence, he inferred that the radius vector E B described areas proportional to the times.

56. From this he deduced a method of calculating the sun's place for any given time. Draw a line E F from the focus of the ellipse, which shall cut off a sector A E F, having the same proportion to the whole surface of the ellipse, which the interval of time between the sun's last passage through his apogee, and the time for which the computation is made, has to a sydereal year; F will be the sun's true place for that time. This is called KEPLER'S PROBLEM.

This problem, the most interesting to astronomers, has not yet been solved otherwise than by approximation, or by geometrical constructions which do not admit of accurate computation.

57. Let A B P D (Fig. 9) be the elliptical orbit, having the earth in the focus E. A and P, the extremities of the transverse axis, are the apogee and perogee of the revolving body. B D is the conjugate axis, and C the centre. It is required to draw a line E T which shall cut off a sector A E T, which has to the whole ellipse the proportion of *m* to *n*; *m* being taken to *n* in the proportion of the time elapsed since the body was in A to the time of a complete revolution.

Kepler, who was an excellent geometer, saw that this would be effected, if he could draw a line E I, which should cut off from the circumscribed circle A *b* P *d* the area A E I, which is to the whole circle in the same proportion of *m* to *n*. For, then, drawing the perpendicular ordinate I R, cutting the ellipse in T, he knew that the area A E T has the same proportion to the ellipse that A E I has to the circle. The proof of this is easy, and it seems greatly to simplify the problem. Draw I C through the centre, and make E S perpendicular to I C S. The area A E I consists of the circular sector A C I, and the triangle C I E. The sector is equal to half the rectangle of the radius C I and the arch A I, that is, to $\frac{CA \times IA}{2}$. The triangle C I E is equal to $\frac{CI \times ES}{2}$, or $\frac{CA \times ES}{2}$. Therefore it is evident that, if we make the arch I M equal to the straight line E S, the sector A C M will be equal to the circular area A C I, and the angle A C M will be to 360 degrees, as *m* to *n*.

58. Hence we see, that it will be easy to find the time when the revolving body is in any point T. To find this, draw the ordinate R T I; draw I C S and E S, and make I M = E S. Then, 360° is to the arch A M as the time of a revolution to the time in which the body moves over A T. This is (in the astronomical language) finding the mean anomaly when the true anomaly is given. The angle A C M, proportional to the time, is called the MEAN ANOMALY, and the angle A E I is the TRUE ANOMALY. The angle A C I is called the ANOMALY OF THE ECCENTRIC, or the ECCENTRIC ANOMALY.

59. But the astronomer wants the true anomaly corresponding to a given mean anomaly. The process here given cannot be reversed. We cannot tell how much to cut off from the given mean anomaly A M, so as to leave A I of a proper magnitude, because the indispensable measure of M I, namely E S, cannot be had till I C S be drawn. Kepler saw this, and said that his problem could not be solved geometrically. Since the invention of fluxions, however, and of converging serieses, very accurate solutions have been obtained. That given by *Frisius* in his *Cosmographia* is the same in principle with all the most approved methods, and the form in which it is presented is peculiarly simple and neat. But, except for the construction of original tables, these methods are rarely employed, on account of the laborious calculation which they require. Of all the direct approximate solutions, that given by Dr Matthew Stewart at the end of his *Tracts, Physical and Mathematical*, published in 1761, seems the most accurate

and elegant; and the calculations founded on it are even shorter than the indirect methods generally employed. His construction is as follows.

60. Let the angle A E M be the mean anomaly, join E M, and draw C *i* parallel to it, and M O perpendicular to C *i*. If the orbit is not more eccentric than that of Mars, make the arch *i* I equal to the excess of the arch M *i* above its sine M O. Then A I is the eccentric anomaly corresponding to the mean anomaly A M, and the ordinate I R will cut the ellipse in T, so that A E T will be the true anomaly required. The error will not amount to two seconds in any part of such orbits. But, for orbits of greater eccentricity, another step is necessary. Join E *i*, and draw C Q parallel to E *i*, meeting the tangent *i* Q in Q. Let D represent the excess of the arch M *i* above its sine M O*, and institute the following analogy, sin. M C *i* : tan. *i* C Q = D : *i* I, taking *i* I from *i* towards M. The point, I, will be so situated that the sector A E I is very nearly equal to the sector A C M, or A I is the eccentric anomaly corresponding to the mean anomaly A M. The error will not amount to one second, even in the orbit of Mercury.

The demonstration of this construction is by no means abstruse or difficult. Draw I S, and M I. The triangles *i* C E and *i* C M are evidently equal, being on one base *i* C, and between the parallels *i* C and M E. For similar reasons, the triangles *i* S I and *i* E I are equal. Therefore the triangle *i* C E, together with the segment included between the arch M *b* *i* and the chord M *i*, will be equal to the circular sector *i* C M.

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* This excess must be expressed in degrees, minutes, or seconds. The radius of a circle is equal to an arch of 206,265 seconds. The logarithm of this number is 5.3144251. Therefore we shall obtain E S, or the seconds in E S, by adding this logarithm to the logarithms of E C (A C being unity), and the logarithm of the sine of A C I. The sum is the logarithm of the seconds in E S.

Now it is plain, from the construction, that $S\,i : C\,i = S\,E : i\,Q, = M\,O : i\,Q, = \overline{M\,b\,i - M\,O} : i\,I$. Therefore $S\,i \times i\,I = C\,i \times M\,b\,i - C\,i \times M\,O$. But $C\,i \times M\,b\,i$ is equal to twice the sector $M\,C\,i$, and $C\,i \times M\,O$ is equal to twice the triangle $M\,C\,i$. Therefore, $S\,i \times i\,I$ is equal to twice the segment contained between $M\,b\,i$ and the chord $M\,i$. Therefore this segment is equal to the triangle $i\,S\,I$, or to the triangle $i\,E\,I$. Therefore the space $C\,i\,I\,E\,C$ is equal to the sector $i\,C\,M$, and the sector $A\,E\,I$ to the sector $A\,C\,M$.

The calculation founded on this construction is extremely simple. In the triangle $M\,C\,E$, the sides $M\,C$ and $C\,E$ are given, with the included angle $M\,C\,E$; and the angles $C\,E\,M$, $C\,M\,E$ are sought. Moreover, $A\,E$ is the sum of the given sides, and $P\,E$ is their difference, and $A\,C\,M$ is the sum of the angles M and E. Therefore $A\,E : E\,P = \tan. \frac{E+M}{2} : \tan. \frac{E-M}{2}$; and thus E and M, or their equals, $A\,C\,i$ and $M\,C\,i$, are obtained. In the next place, in the triangle $i\,C\,E$, the sides $i\,C$, $C\,E$, and the included angle $i\,C\,E$, are given, and the angle $E\,i\,C$ is sought. We have in the same manner as before, $A\,C\,i$ equal to the sum of the angles E and i, and therefore $A\,E : E\,P = \tan. \frac{E+i}{2} : \tan. \frac{E-i}{2}$. Thus the angle $E\,i\,C$, or its equal, $i\,C\,Q$, is obtained, and then, the arch $i\,I = D \times \frac{i\,Q}{M\,O}$, $= D \times \frac{\tan.\, i\,C\,Q}{\sin.\, M\,C\,i}$.

In the very eccentric orbits of the comets, this brings us vastly nearer to the truth than any of the indirect methods we know does by the first step. So near, indeed, that the common method, by the *rule of false position*, may now be safely employed. If the point, I, has been accurately found, it is plain, that if to the arch $A\,I$ we add $E\,S$, that is, $E\,C \times \sin.\, A\,C\,I$, we obtain the arch $A\,M$ with which we began. But if I has not been accurately determined, $A\,M$

will differ from the primitive A M. Therefore, make some small change on A I, and again compute A M. This will probably be again erroneous. Then apply the rule of false position as usual. The error remaining after the first step of Dr Stewart's process is always so moderate, that the variations of A M are very nearly proportional to the variations of A I; so that two steps of the rule will generally bring the calculation within two or three seconds of the truth. The astronomical student will find many beautiful and important propositions in these mathematical tracts. The proposition just now employed is in page 398, &c.

61. Astronomers have discovered, that the line A P moves slowly round E to the eastward, changing its place about 25′ 56″ in a century. This makes the time of a complete revolution in the orbit to be 365^d 6^h 15′ 20″. This time is called the ANOMALISTIC YEAR.

Of the proper Motions of the Moon.

62. Of all the celestial motions, the most obvious are those of the Moon. We see her shift her situation among the stars about her own breadth to the eastward in an hour, and in somewhat less than a month she makes a complete tour of the heavens. The gentle beauty of her appearance during the quiet hours of a serene night, has attracted the notice, and we may say the affections of all mankind; and she is justly styled the Queen of Heaven. The remarkable and distinct changes of her appearance have afforded to all simple nations a most convenient index and measure of time, both for recording past events, and for making any future appointments for business. Accordingly, we find, in the first histories of all nations, that the lunar motions were the first studied, and, in some degree, understood. It seems to have been in subserviency to this study alone that the other appearances of the starry heavens were attended to; and the relative positions of the stars

seem to have interested us, merely as the means of ascertaining the motions of the Moon. For we find all the zodiacs of the ancient oriental nations, divided, not into 12 equal portions, corresponding to the Sun's progress during the period of seasons, but into 27 parts, corresponding to the Moon's daily progress, and these are expressly called the HOUSES or MANSIONS of the Moon. This is the distribution of the zodiac of the ancient Hindoos, the Persians, the Chinese, and even the Chaldeans. Some have no division into 12, and those who have, do not give *names* to 12 groups of stars, but to 27. They first describe the situation of a planet in one of these mansions by name, noting its distance from some stars in that group, and thence infer in what part of which twelfth of the circumference it is placed. The division into 12 parts is merely mathematical, for the purpose of calculation. In all probability, therefore, this was an after-thought, the contrivance of a more cultivated age, well acquainted with the heavens as an object of sight, and beginning to extend the attention to speculations beyond the first conveniences of life.

63. When the Moon's path through this series of mansions is carefully observed, it is found to be (very nearly) a great circle of the heavens, and therefore in a plane passing through the centre of the earth.

64. She makes a complete revolution of the heavens in $27^d\ 7^h\ 43'\ 12''$, but with some variations. Her mean daily motion is therefore 13° 10′ 25″, and her horary motion is 32′ 56″.

65. Her orbit is inclined to the plane of the ecliptic in an angle of 5° 8′ 45″, nearly, cutting it in two points called her NODES, diametrically opposite to each other; and that node through which she passes in coming from the south to the north side of the ecliptic, is called the ASCENDING NODE.

66. The nodes have a motion which is generally westward, but with considerable irregularities, making a complete revolution in about $6803^d\ 2^h\ 55'\ 18''$, nearly 18⅔ years.

67. If we mark on a celestial globe a series of points where the Moon was observed during three or four revolutions, and then lap a tape round the globe, covering these points, we shall see that the tape crosses the ecliptic more westerly every turn, and then crosses the last round very obliquely; and we see that by continuing this operation, we shall completely cover with the tape a zone of the heavens, about ten or eleven degrees broad, having the ecliptic running along its middle.

68. The Moon moves unequally in this orbit, her hourly motion increasing from 29′ 34″ to 36′ 48″, and the equation of the orbit sometimes amounts to 6° 18′ 32″; so that if, setting out from the point where her horary motion is slowest, we calculate her place, for the eighth day thereafter, at the rate of 32′ 56″ per hour, we shall find her observed place short of our calculation more than half a day's motion. And we should have found her as much before it, had we begun our calculation from the opposite point of her orbit.

69. Her apparent diameter changes from 29′ 26″ to 33′ 47″, and therefore her distance from the Earth changes. This distance may be discovered in miles by means of her parallax.

She was observed, in her passage over the meridian, by two astronomers, one of whom was at Berlin, and the other at the Cape of Good Hope. These two places are distant from one another above 5000 miles; so that the observer at Berlin saw the Moon every day considerably more to the south than the person at the Cape. This difference of apparent declination is the measure of the angle D S C (Fig. 6.) subtended at the Moon by the line *c* D of 5443 miles, between the observers. The angles S D *c* and S *c* D are given by means of the Moon's observed altitudes. Therefore any of the sides S D or S *c* may be computed. It is found to be nearly 60 semidiameters of the earth.

70. By combining the observations of the Moon's place in the heavens with those of her apparent diameter, we dis-

cover that her orbit is nearly an ellipse, having the Earth in one focus, and having the longer axis to the shorter axis nearly as 91 to 89. The greatest and least distances are nearly in the proportion of 21 to 19.

71. Her motion in this ellipse is such, that the line joining the Earth and Moon describes areas which are nearly proportional to the times. For her angular hourly motion is observed to be as the square of her apparent diameter.

72. The line of the apsides has a slow motion eastward, completing a revolution in about $3232^d\ 11^h\ 14'\ 30''$, nearly 9 years.

73. While the Moon is thus making a revolution round the heavens, her appearance undergoes great changes. She is sometimes on our meridian at midnight, and, therefore, in the part of the heavens which is opposite to the Sun. In this situation, she is a complete luminous circle, and is said to be FULL. As she moves eastward, she becomes deficient on the west side, and, after about 7½ days, comes to the meridian about six in the morning, having the appearance of a semicircle, with the convex side next the Sun. In this state, her appearance is called HALF MOON. Moving still eastward, she becomes more deficient on the west side, and has now the form of a crescent, with the convex side turned towards the Sun. This crescent becomes continually more slender, till, about 14 days after being full, she is so near the Sun, that she cannot be seen on account of his great splendour. About four days after this disappearance in the morning before sunrise, she is seen in the evening, a little to the eastward of the Sun, in the form of a fine crescent, with the convex side turned towards the Sun. Moving still to the eastward, the crescent becomes more full; and when the Moon comes to the meridian about six in the evening, she has again the appearance of a bright semicircle. Advancing still to the eastward, she becomes fuller on the east side, and at last, after about 29½ days, she is again opposite to the Sun, and again full.

74. It frequently happens, that the Moon is ECLIPSED when full; and that the Sun is eclipsed some time between the disappearance of the Moon in the morning on the west side of the Sun, and her reappearance in the evening on the east side of the Sun. This eclipse of the Sun happens at the very time that the Moon, in the course of her revolution, passes that part of the heavens where the Sun is.

75. From these observations we conclude, 1. That the Moon is an opaque body, visible only by means of the Sun's light illuminating her surface; 2. That her orbit round the Earth is nearer than the Sun's.

76. From these principles all her PHASES, or appearances, may be explained (Fig. 10.)

77. When the Moon comes to the meridian at mid-day, she is said to be NEW, and to be in CONJUNCTION with the Sun. When she comes to the meridian at midnight, she is said to be in OPPOSITION. The line joining these two situations is called the line of the SYZIGIES. The points where she is half-illuminated are called the QUADRATURES; and that is called the first quadrature which happens after new Moon.

78. When the Moon is half illuminated, the line E M (Fig. 10.) joining the Earth and Moon, is perpendicular to the line M S, joining the Moon and Sun. By observing the angle S E M, the proportion of the distance of the Sun to the distance of the Moon may be ascertained.

This method of ascertaining the Sun's distance was proposed by Aristarchus of Samos, about 264 years before the Christian era. The thought was extremely ingenious, and strictly just; and this was the first observation that gave the astronomers any confident guess at the very great distance of the Sun. But it is impossible to judge of the half-illumination of the Moon's disk with sufficient accuracy for obtaining any tolerable measure. Even now, when assisted by telescopes, we cannot tell to a few minutes when the boundary between light and darkness in the Moon is exactly a straight line. When this really happens, the elongation S E M wants but 9′ of a right angle, and when it is altogether a right angle, there is no sensible change in the appearance of the Moon. All that the ancient astronomers could infer, from the best estimation of the bisection of the Moon, was, that the Sun was, for certain, at a much greater distance than any person had supposed before that time. Aristarchus said, that the angle S E M was not less than 87 degrees, and therefore the Sun was at least twenty times farther off than the Moon. But astronomers of the Alexandrian school said, that the angle S E M exceeded 89°, and the Sun was sixty times more remote than the Moon. Modern observations shew him to be near four hundred times more remote.

79. This suggestion of phases is completed in a period of $29^d\ 12^h\ 44'\ 3''$, called a SYNODICAL MONTH and a LUNATION.

It may be asked here, how the period of a lunation comes to differ from that of the Moon's revolution round the Earth, which is accomplished in $27^d\ 7^h\ 43'\ 12''$? This is owing to the Sun's change of place during a revolution of the Moon. Suppose it new Moon, and therefore the Sun and Moon appearing in the same place of the heavens. At the end of the lunar period, the Moon is again in that point of the heavens. But the Sun, in the mean time, has advanced above 27 degrees; and somewhat more than two days must elapse before the Moon can overtake the Sun, so as to be seen by us as new Moon.

80. The period of this succession of phases may be found within a few hours of the truth in a very short time. We can tell, within four or five hours, the time of the Moon being half-illuminated. Suppose this observed in the morning of her last quarter. We shall see this twice repeated in 59 days, which gives $29^d\ 12^h$ for a lunation, wanting about three-fourths of an hour of the truth. About 433 years before the Christian era, Meton, a Greek

astronomer, reported to the states assembled at the Olympic games, that in nineteen years there happened exactly 235 lunations.

81. The lunar motions are subject to several irregularities, of which the following are the chief:

82. 1. The periodic month is greater when the Sun is in perigee than when in apogee, the greatest difference being about 24 minutes. Tycho Brahé first remarked this anomaly of the lunar motions, and called the correction (depending on the Sun's place in his orbit) the ANNUAL EQUATION of the Moon.

83. 2. The mean period is less than it was in ancient times.

84. 3. The orbit is larger when the Sun is in perigee than when he is in apogee.

85. 4. The orbit is more eccentric when the Sun is in the line of the lunar apsides; and the equation of the orbit is then increased nearly 1° 20′ 34″. This change is called the EVECTION. It was discovered by Ptolemy.

86. 5. The inclination of the orbit changes.

87. 6. The Moon's motion is retarded in the first and third quarters, and accelerated in the second and last. This anomaly was discovered by Tycho Brahé, who calls it the VARIATION.

88. 7. The motion of the nodes is very unequal.

Of the Kalendar.

89. Astronomy, like all other sciences, was first practised as an art. The chief object of this art was to know the seasons, which, as we have seen, depend either immediately, or more remotely, on the Sun's motion in the ecliptic. A ready method for knowing the season seems, in all ages, to have been the chief incitement to the study of astronomy. This must direct the labours of the field, the migrations of the shepherd, and the journies of the traveller.

It is equally necessary for appointing all public meetings, and for recording events.

Were the stars visible in the day-time, it would be easy to mark all the portions of the year by the Sun's place among them. When he is on the foot of Castor, it is midsummer; and midwinter, when he is on the bow of Sagittarius. But this cannot be done, because his splendour eclipses them all.

90. The best approximation which a rude people can make to this, is to mark the days in which the stars of the zodiac come first in sight in the morning, in the eastern horizon, immediately before the Sun rise. As he gradually travels eastward along the ecliptic, the brighter stars which rise about three quarters of an hour before the Sun, may be seen in succession. The husbandman and the shepherd were thus warned of the succeeding tasks by the appearance of certain stars before the Sun. Thus, in Egypt, the day was proclaimed in which the Dogstar was first seen by those set to watch. The inhabitants immediately began to gather home their wandering flocks and herds, and prepare themselves for the inundation of the Nile in twelve or fourteen days. Hence that star was called the *Watch-dog*, THOTH, the Guardian of Egypt.

This was therefore a natural commencement of the period of seasons in Egypt; and the interval between the successive apparitions of Thoth, has been called the NATURAL year of that country, to distinguish it from the civil or artificial year, by which all records were kept, but which had little or no alliance with the seasons. It has also been called the *Canicular* year. It evidently depends on the Sun's situation and distance from the Dog-star, and must therefore have the same period with the Sun's revolution from a star to the same star again. This requires $365^d\ 6^h\ 9'\ 11''$, and differs from our period of seasons. Hence we must conclude, that the rising of the Dog-star is not an infallible presage of the inundation, but will be found faulty after a long course of ages. At present it happens about the 12th or 11th of July.

This observation of a star's first appearance in the year, by getting out of the dazzling blaze of the Sun, is called the *heliacal rising* of the star. The ancient almanacks for directing the rural labours were obliged to give the detail of these in succession, and of the corresponding labours. Hesiod, the oldest poet of the Greeks, has given a very minute detail of those heliacal risings, ornamented by a pleasing description of the successive occupations of rural life. This evidently required a very considerable knowledge of the starry heavens, and of the chief circumstances of diurnal motion, and particularly the number of days intervening between the first appearance of the different constellations.

Such an almanack, however, cannot be expected, except among a somewhat cultivated people, as it requires a long continued observation of the revolution of the heavens in order to form it; and it must, even among such people, be uncertain. Cloudy, or even hazy weather, may prevent us for a fortnight from seeing the stars we want.

91. The Moon comes most opportunely to the aid of simple nations, for giving the inhabitants an easy division and measure of time. The changes in her appearance are so remarkable, and so distinct, that they cannot be confounded. Accordingly, we find that all nations have made use of the lunar phases to reckon by, and for appointing all public meetings. The festivals and sacred ceremonies of simple nations were not all dictated by superstition; but they served to fix those divisions of time in the memory, and thus gave a comprehensive notion of the year. All these festivals were celebrated at particular phases of the Moon—generally at new and full Moon. Men were appointed to watch her first appearance in the evening, after having been seen in the morning, rising a few minutes before the Sun. This was done in consecrated groves, and in high places; and her appearance was *proclaimed*.

Fourteen days after, the festival was generally held *during full Moon*. Hence it is that the first day of a Roman month was named KALENDÆ, the day *to be proclaimed*. They said *pridie*, *tertio*, *quarto*, &c. *ante calendas neomenias Martias*; the third, fourth, &c. before proclaiming the new Moon of March. And the assemblage of months, with the arrangement of all the festivals and sacrifices, was called a KALENDARIUM.

As superstition overran all rude nations, no meeting was held without sacrifices and other religious ceremonies—the watching and proclaiming was naturally committed to the priests—the kalendar became a sacred thing, connected with the worship of the gods—and, long before any moderate knowledge of the celestial motions had been acquired, every day of every Moon had its particular sanctity, and its appropriated ceremonies, which could not be transferred to any other.

92. But as yet there seemed no precise distinction of months, nor of what number of months should be assembled into one group. Most nations seem to have observed that, after 12 Moons were completed, the season was pretty much the same as at the beginning. This was probably thought exact enough. Accordingly, in most ancient nations, we find a year of 354 days. But a few returns of the winter's cold, when they expected heat, would shew that this conjecture was far from being correct; and now began the embarrassment. There was no difficulty in determining the period of the seasons exactly enough, by means of very obvious observations. Almost any cottager has observed that, on the approach of winter, the Sun rises more to the right hand, and sets more to the left every day, the places of his rising and setting coming continually nearer to each other; and that, after rising for two or three days from behind the same object, the places of rising and setting again gradually separate from each other. By such familiar observations, the experience of an ordinary life is sufficient for determining the period of the seasons with abundant accuracy. The difficulty was to accomplish the reconciliation of this period with the sacred cycle of months, each day of which was consecrated to a particular deity, jealous of his honours. Thus the Hierophantic science, and the whole art of kalendar-making, were necessarily entrusted to the priests. We see this in the history of all nations, Jews, Pagans, and Christians.

93. Various have been the contrivances of different nations. The Egyptians, and some of the neighbouring Orientals, seem early to have known that the period of seasons considerably exceeded 12 months, and contained 365 days. They made the civil year consist of 12 months of 30 days, and added 5 complimentary days without ceremonies; and when more experience convinced them that the year contained a fraction of a day more, they made no change, but made the people believe that it was an improvement on their kalendar, that their great day, the first of *Thoth*, by falling back one day in four periods of seasons, would thus occupy in succession every day of the year, and thus sanctify the whole in 1461 years, as they imagined, but really in 1425 of their civil years. We have but a very imperfect knowledge of the arrangement of their festivals. Indeed they were totally different in almost every city.

It is important to the astronomer to know this method of reckoning; because all the observations of Hipparchus and Ptolemy, and all those which they have quoted from the Chaldeans, Persians, &c. are recorded by it. In An. Dom. 940, the first day of Thoth fell on the first of January, and another Egyptian year commenced on the 31st of December of that year. From this datum it is easy to reckon back by years of 365 days, and to say on what day of what month of any of our years the first day of Thoth falls, and this wandering year commences.

94. The Greeks have been much more puzzled with the

formation of a lunisolar year than the Egyptians. Solon got an oracle to direct his Athenians (594 years before our æra), ἀγειν κατα τρια, κατα Ἡλιον, κατα Σεληνην, και κατα ἡμερας. The meaning of which seems to be, to regulate their year by the Sun, or seasons, their months by the Moon, and their festivals by the days. Observing that 59 days made two months, he made these alternately of 30 and of 29 days, πληρεις and κοιλοι, full, and deficient; and the 30th day of a month, the τριακας, was called ενη και νεα, νεομηνια, as it belonged to both months.

But this was not sufficiently accurate; and the Olympic games, celebrated on every fourth year, during the full Moon nearest to midsummer day, had gone into great confusion. The Hierophants, whose proclamation to all the states assembled the chiefs together, had not skill enough to keep them from gradually falling into the autumn months. Injudicious corrections were made from time to time, by rules for inserting months to bring things to rights again. It deserves to be remarked here, that this is the way in which the ancient astronomy improved, before the establishment of the Alexandrian school. It was not by a more accurate observation of the motions, as in modern times, but by discovering the errors, when they amounted to an unit of the scale on which they were measured. The astronomers then improved their future computations by repeatedly cutting off this unit of accumulated error.

95. All these contrivances were publicly proposed at the meeting of the States for the Olympic Games. This was an occasion peculiarly proper, and here the scheme of Meton was received with just applause. For Meton not only gave his countrymen a very exact determination of the lunar month, but accompanied it with a scheme of intercalation, by which all their festivals, religious and civil, were arranged so as to have very small dislocations from the days of new and full Moon. As this had hitherto been a matter of insuperable difficulty, Meton was declared victor in the

first department, a statue was decreed him, and his arrangement of the festivals was inscribed on a pillar of marble, in letters of gold. This has occasioned the number expressing the current year of the cycle of 19 years (called the Metonic cycle) to be called the Golden Number. This scheme of Meton's was indeed very judicious, though intricate, because he arranged the interpolation of a month so as never to remove the first day of the month two days from the time of new Moon, whereas it had often been a week.

The Metonic cycle commenced on 16th July, 433 years before the beginning of the Christian æra, at 43 minutes past seven in the morning, that being the time of new Moon. The first year of each cycle is that in which the full Moon of its first month is the nearest to the summer solstice.

96. The Roman kalendar was in a much worse condition than the rudest of the Greeks. The superstitious veneration for their ceremonies, or their passion for public sports, had diverted the attention of the Romans (who never were cultivators or graziers) from the seasons altogether. They were contented with a year of ten months for several centuries, and had the most absurd contrivances for producing some conformity with the seasons. At last, that accomplished general, Julius Cæsar, having attained the height of his vast ambition, resolved to reform the Roman kalendar. He was profoundly skilled in astronomy, and had written some dissertations on different branches of the science, which had great reputation, but are now lost. He had no superstitious or religious qualms to disturb him, and was determined to make every thing yield to the great purpose of a kalendar, its use in directing the occupations of the people, and for recording the events of history. He took the help of Sosigenes, an astronomer of the Alexandrian school, a man perfectly acquainted with all the dis-

coveries of Hipparchus and others of that celebrated academy.

These eminent scholars, knowing that the period of seasons occupied 365 days and a quarter very nearly, made a short cycle of 4 years, containing three years of 365, and one of 366 days; thus cutting off, in the Grecian manner, the error, when it amounted to a whole day. Cæsar resolved also to change the beginning of the year from March, where Romulus had placed it in honour of his patron Mars, to the winter solstice. This is certainly the most natural way of estimating the commencement of the year of seasons. What we are most anxious to ascertain is the precise day when the Sun, after having withdrawn his cheering beams, and exposed us to the uncomfortable cold and storms of winter, begins to turn toward us, and to bring back the pleasures of spring, and by his genial warmth to give us the hopes of another season of productive fertility. Cæsar therefore chose for the beginning of his kalendar, a year in which there was a new Moon following close upon the winter solstice. This opportunity was afforded him in the second year of his dictatorship, and the 707th year from the foundation of Rome. He found that there would be a new Moon six days after the winter solstice. He made this new Moon the 1st of January of his first year. But, to do this, he was obliged to keep the preceding year dragging on 90 days longer than usual, containing 444 days, instead of the old number 354. As all these days were unprovided with solemnities, the year preceding Cæsar's calendar was called *the year of confusion*. Cæsar also, for a particular reason, chose to make his first year consist of 366 days, and he inserted the intercalary day between the 23d and 24th of February, choosing that particular day, as a separation of the lustrations and other piaculums to the infernal deities, which ended with the 23d, from the worship of the celestial deities, which took place on the 24th of February. The 24th was the *sextus ante kalendas neomenias Martias*. His inserted day, answering civil purposes alone, had no ceremonies, nor any name appropriated to it, and was to be considered merely as a supernumerary *sextus ante kalendas*. Hence the year which had this intercalation was styled an *annus bissextilis*, a bissextile year. With respect to the rest of the year, Cæsar being also Pontifex Maximus (an office of vast political importance), or rather, having all the power of the state in his own person, ordered that attention should be given to the days of the month only, and that the religious festivals alone should be regulated by the sacred college. He assigned to each month the number of days which has been continued in them ever since.

97. Such is the simple calendar of Julius Cæsar. Simple however as it was, his instructions were misunderstood, or not attended to, during the horrors of the civil wars. Instead of intercalating every fourth year, the intercalation was thrice made on every succeeding third year. The mistake was discovered by Augustus, and corrected in the best manner possible, by omitting three intercalations during the next twelve years. Since that time, the kalendar has been continued without interruption over all Europe till 1582. The years, consisting of 365¼ days, were called *Julian years*; and it was ordered, by an edict of Augustus, that this kalendar shall be used through the whole empire, and that the years shall be reckoned by the reigns of the different emperors. This edict was but imperfectly executed in the distant provinces, where the native princes were allowed to hold a vassal sovereignty. In Egypt particularly, although the court obeyed the edict, the people followed their former kalendars and epochs. Ptolemy the astronomer retains the reckoning of Hipparchus, by Egyptian years, reckoned from the death of Alexander the Great. We must understand all these modes of computation, in order to make use of the ancient astronomical observations. A comparison of the different epochs will be given as we finish the subject.

98. The era adopted by the Roman Empire when Christianity became the religion of the state, was not finally settled till a good while after Constantine. Dionysius Exiguus, a French monk, after consulting all proper documents, considers the 25th of December of the forty-fifth year of Julius Cæsar as the day of our Saviour's nativity. The 1st of January of the forty-sixth year of Cæsar is therefore the beginning of the era now used by the Christian world. Any event happening in this year is dated *anno Domini primo.* As Cæsar had made his first year a bissextile, the year of the nativity was also bissextile; and the first year of our era begins the short cycle of four years, so that the fourth year of our era is bissextile.

That we may connect this era with all the others employed by astronomers or historians, it will be enough to know that this first year of the Christian era is the 4714th of the Julian period.

It coincides with the fourth year of the 194th Olympiad till midsummer.

It coincides with the 753d *ab urbe condita,* till April 21st.

It coincides with the 748th of Nabonassar till August 23d.

It coincides with the 324th civil year of Egypt, reckoned from the death of Alexander the Great.

In the arrangement of epochs in the astronomical tables, the years before the Christian era are counted backwards, calling the year of the nativity 0, the preceding year 1, &c. But chronologists more frequently reckon the year of the nativity the first before Christ. Thus,

Years of Cæsar…….	41,	42,	43,	44,	45,	46,	47,	48,	49
Astronomers……….	4,	3,	2,	1,	0,	1,	2,	3,	4
Chronologists………	5,	4,	3,	2,	1,	1,	2,	3,	4

This kalendar of Julius Cæsar has manifest advantages in respect of simplicity, and in a short time supplanted all others among the western nations. Many other nations had perceived that the year of seasons contained more than 365 days, but had not fallen on easy methods of making the correction. It is a very remarkable fact, that the Mexicans, when discovered by the Spaniards, employed a cycle which supposed that the year contained 365¼ days. For, at the end of fifty-two years, they add thirteen days, which is equivalent to adding one every fourth year. In their hieroglyphical annals, their years are grouped into parcels of four, each of which has a particular mark.

99. But although the Julian construction of the civil year greatly excelled all that had gone before, it was not perfect, because it contained 11′ 14½″ more than the period of seasons. This, in 128 years, amounts exactly to a day. In 1582, it amounted to $12^d\ 7^h$. The equinoxes and solstices no longer happened on those days of the month that were intended for them. The celebration of the church festivals was altogether deranged. For it must now be remarked, that there occurred the same embarrassment on account of the lunar months, as formerly in the Pagan world.

The Council of Nice had decreed that the great festival, Easter, should be celebrated in conformity with the Jewish passover, which was regulated by the new Moon following the vernal equinox. All the principal festivals are regulated by Easter Sunday. But by the deviation of the Julian kalendar from the seasons, and the words of the decree of the Nicene Council, the celebration of Easter lost all connexion with the Passover. For the decree did not say, 'The first Sunday after the full Moon following the vernal equinox, but the first Sunday after the full Moon following the 21st of March.' It frequently happened that Easter and the Passover were six weeks apart. This was corrected by Pope Gregory the XIII. in 1582, by bringing the 21st of March to the equinox again. He first cut off the ten days which had accumulated since the Council of Nice; and, to prevent this accumulation, he directed the intercalation of a bissextile to be omitted on every centurial year. But the error of a Julian century containing 36525 days, is not a whole day, but $18^h\ 40'$. Therefore the correction introduces an error of $5^h\ 20'$. To prevent this from accumulating, the omission of the centurial intercalation is limited to the centuries not divisible by four. Therefore 1600, 2000, 2400, &c. are still bissextile years; but 1700, 1800, 1900, 2100, 2200, &c. are common years; There still remains an error, amounting to a day in 144 centuries.

The kalendar is now sufficiently accurate for all purposes of history and record, and even for astronomy, because the tropical year of seasons is subject to a periodical inequality.

100. A correction, much more accurate than the Gregorian, occurred to Omar, a Persian astronomer at the court of Prince Gelala Eddin Melek Schah. Omar proposed always to delay to the thirty-third year, the intercalation which should have been made in the thirty-second. This is equivalent to omitting the Julian intercalation altogether on the 128th year. This method is extremely simple, and scrupulously accurate. For the error of 11′ 15″ of the Julian year amounts precisely to a day in 128 years. It differs from the truth only one minute in 120 years. This correction took place in A. D. 1079, at the same time that the Arab Alhazen was reforming the science of astronomy in Spain.

The Gregorian calendar, however, has less chance of being forgotten or mistaken. Centurial years are remarkable, and call the attention, even by the unusual sound of the words. The thirty-second year has nothing remarkable, and may be overlooked.

101. It now appears that certain attentions are necessary for avoiding mistakes, when we would appeal to very distant observations. We must know the accurate interval, however large. Although one hundred Julian years contain 36,525 days, we must keep in mind, that between

1500 and 1600 ten days are wanting; and that each of the centuries 1700 and 1800 also want a day. The interval from the beginning of our era, and A. D. 1582, needs no attention; but that between 1505 and 1805 wants twelve days of three Julian centuries.

102. We must also be careful, in using the ancient observations, to connect the years of our Lord with the years before Christ in a proper manner. An eclipse mentioned by an astronomer as having happened on the 1st of February, anno 3tio A. C. must be considered as happening in the forty-second year of Julius Cæsar. But if the same thing is mentioned by a historian or chronologist, it is much more probable that it was in the forty-third year of Cæsar. It was chiefly to prevent all ambiguities of this kind that Scaliger contrived what he called the *Julian period*. This is a number made by multiplying together the numbers called the *Lunar, or Metonic cycle*, the *solar cycle*, and the *indiction*. The lunar cycle is 19, and the first year of our Lord was the second of this cycle. The solar cycle is 28, being the number of years in which the days of the month return to the same days of the week. As the year contains fifty-two weeks and one day, the first day of the year (or any day of any month) falls back in the week one day every year, till interrupted by the intercalation in a bissextile year. This makes it fall back two days in that year; and therefore it will not return to the same day till after four times seven, or twenty-eight years. The first year of our Lord was the tenth of this cycle. The INDICTION is a cycle of fifteen years, at the beginning of which a tax was levied over the Roman empire. It took place A. D. 312; and if reckoned backward, it would have begun three years before the Christian era. The year of this cycle, for any year of the Christian era, will therefore be had by adding three to the year, and dividing by fifteen. The product of these three numbers is 7980; and it is plain, that this number of years must elapse before a

year can have the same place in all the three cycles. If, therefore, we know the place of these cycles, belonging to any year, we can tell what year it is of the Julian period.

The first year of our era was the second of the lunar cycle, the tenth of the solar, and the fourth of indiction, and the 4714th of the Julian period. By this we may arrange all the remarkable eras as follows:

	J. P.	⊙	☽	I.	A. C.
Era of the Olympiads	3938	18	5	8	775,776
Foundation of Rome	3961	13	9	1	752,753
Nabonassar	3967	19	15	7	746,747
Death of Alexander	4390				323,324
First of Julius Cæsar	4669	21	14	4	44, 45
A. Dom. 1	4714	10	2	4	

103. Did the Metonic cycle of the Moon correspond exactly with our year, it would mark for any year the number of years which have elapsed since it was new Moon on the 1st of January. But its want of perfect accuracy, the vicinity of an intercalation, and the lunar equations, sometimes cause an error of two days. It is much used, however, for ordinary calculations for the Church holidays. To find the golden number, add one to the year of our Lord, divide the sum by 19, the remainder is the golden number. If there be no remainder, the golden number is 19.

104. Another number, called Epact, is also used for facilitating the calculation of new and full Moon in a gross way. The epact is nearly the Moon's age on the 1st of January. To find it, multiply the golden number by 11, add 19 to the product, and divide by 30. The remainder is the epact.

Knowing, by the epact, the Moon's age on the 1st of January, and the day of the year corresponding to any day of a month, it is easy to find the Moon's age on that day, by dividing the double of the sum of this number and the epact by 59. The half remainder is nearly the Moon's age.

Although these rude computations do not correspond with the motions of the two luminaries, they deserve notice, being the methods employed by the rules of the church for settling the moveable church festivals.

Of the proper Motions of the Planets.

105. The planets are observed to change their situations in the starry heavens, and move among the signs of the zodiac, never receding far from the ecliptic.

Their motions are exceedingly irregular, as may be seen by Fig. 11, which represents the motion of the planet Jupiter, from the beginning of 1708 to the beginning of 1710. E K represents the ecliptic, and the initial letters of the months are put to those points of the apparent path where the planet was seen on the first day of each month.

It appears that, on the 1st of January 1708, the planet was moving slowly eastward, and became stationary about the middle of the month, in the second degree of Libra. It then turned westward, gradually increasing its westerly motion, till about the middle of March, when it was in opposition to the Sun, at R, all the while deviating farther from the ecliptic toward the north. It now slackened its westerly motion every day, and was again stationary about the 20th of May, in the twenty-second degree of Virgo, and had come nearer to the ecliptic. Jupiter now moved eastward, nearly parallel to the ecliptic, gradually accelerating in his motion, till the beginning of October, when he was in conjunction with the Sun at D, about the eleventh degree of Libra. He now slackened his progressive motion every day, till he was again stationary, in the second degree of Scorpio, on the 12th or 13th of February 1709. He then moved westward, was again in opposition, in the twenty-seventh degree of Libra, about the middle of April.

He became stationary, about the end of June, in the twenty-first degree of Libra; and from this place he again proceeded eastward; was in conjunction about the beginning of November, very near the star in the southern scale of Libra; and, on the 1st of January 1710, he was in the twenty-fourth degree of Scorpio.

This figure will very nearly correspond with the apparent motions of the planet in the same months of 1803 and 1804. Jupiter will go on in this manner, forming a loop in his path in every thirteenth month; and he is in opposition to the Sun, when in the middle of each loop. His regress in each loop is about 10 degrees, and his progressive motion is continued about 40 degrees. He gradually approaches the ecliptic, crosses it, deviates to the southward, then returns towards it; crosses it, about six years after his former crossing, and in about twelve years comes to where he was at the beginning of these observations.

106. The other planets, and particularly Venus and Mercury, are still more irregular in their apparent motions, and have but few circumstances of general resemblance.

The first *general* remark which can be made on these intricate motions is, that a planet always appears largest when in the points R, R, R, which are in the middle of its retrograde motions. Its diameter gradually diminishes, and becomes the least of all when in the points D′, D′, D′, which are in the middle of its direct motions. Hence we infer, that the planet is nearest to the Earth when in the middle of its retrograde motion, and farthest from it when in the middle of its direct motion.

It may also be remarked, that a planet is always in conjunction with the Sun, or comes to our meridian at noon, when in the middle of its direct motions. The planets Venus and Mercury are also in conjunction with the Sun when in the middle of their retrograde motions. But the planets Mars, Jupiter, and Saturn, are always in opposition to the Sun, or come to our meridian at midnight, when in the middle of their retrograde motions. Their situations also, when stationary, are always similar, relative to the Sun. These appearances in all the planetary motions have therefore an evident relation to the Sun's place.

107. The ancient astronomers were of opinion, that the perfection of nature required all motions to be uniform, as far as the purpose in view would permit. The planetary motions must therefore be uniform, in a figure that is uniform; and the astronomers maintained, that the observed irregularities were only apparent. Their method for reconciling these with their principle of perfection is very obviously suggested by the representation here given of the motion of Jupiter. They taught, that the planet moves uniformly in the circumference of a circle *q r s* (Fig. 12) in a year, while the centre Q of this circle is carried uniformly round the Earth T, in the circumference of another circle Q A L. The circle Q A L is called the DEFERENT CIRCLE, and *q r s* is called the EPICYCLE. They explained the deviation from the ecliptic, by saying, that the deferent and the epicycle were in planes different from that of the ecliptic. By various trials of different proportions of the deferent and the epicycle, they hit on such dimensions as produced the quantity of retrograde motion that was observed to be combined with the general progress in the order of the signs of the zodiac.—But another inequality was observed. The arch of the heavens intercepted between two successive oppositions of Jupiter (for example), was observed to be variable, being always less in a certain part of the zodiac, and gradually increasing to a maximum state in the opposite part of the zodiac.

In order to correspond with this SECOND INEQUALITY, as it was called, and yet not to imply any inequality of the motion of the epicycle in the circumference of the deferent circle, the astronomers placed the Earth not in, but at a certain distance from, the centre of the deferent; so that an equal arch between two succeeding oppositions should subtend a smaller angle, when it is on the other side of that centre. Thus, the unequal motion of the epicycle was explained in the same way as the Sun's unequal motion in his annual orbit. The line drawn through the Earth and the centre of the deferent is called the line of the planet's APSIDES, and its extremities are called the *apogee* and *perigee* of the deferent as in the case of the Sun's orbit (54.) In this manner, they at last composed a set of motions which agreed tolerably well with observation.

The celebrated geometer Apollonius gave very judicious directions how to proportion the epicycle to the deferent circle. But they seem not to have been attended to, even by Ptolemy; and the astronomers remained very ignorant of any method of construction which agreed sufficiently with the phenomena, till about the thirteenth century, when the doctrine of epicycles was cultivated with more care and skill.

A very full and distinct account is given of all the ingenious contrivances of the ancient astronomers for explaining the irregularities of the celestial motions, in the first part of Dr Small's *History of the Discoveries of Kepler*, published in 1803.

Of the Motions of Venus and Mercury.

108. Venus has been sometimes seen moving across the Sun's disk from east to west, in the form of a round black spot, with an apparent diameter of about 59″. A few days after this has been observed, Venus is seen in the morning, rising a little before the Sun, in the form of a fine crescent, with the convexity turned toward the Sun. She moves gradually westward, separating from the Sun, with a retarded motion, and the crescent becomes more full. In about ten weeks, she has moved 46° west of the Sun, and is now a semicircle, and her diameter is 26″. She now separates no farther from the Sun, but moves eastward, with a motion gradually accelerated, and she gradually dimin-

ishes in apparent diameter. She overtakes the Sun, about 9½ months after having been seen on his disk. Some time after, Venus is seen in the evening, east of the Sun, round, but very small. She moves eastward, and increases in apparent diameter, but loses of her roundness, till she gets about 46° east of the Sun, when she is again a semicircle, having the convexity toward the Sun. She now moves westward, increasing in diameter, but becoming a crescent, like the waning Moon; and, at last, after a period of nearly 584 days, comes again into conjunction with the Sun, with an apparent diameter of 59″.

109. From these phenomena we conclude, that the Sun is included within the orbit of Venus, and is not far from its centre, while the Earth is without this orbit. Therefore, while the Sun revolves round the Earth, Venus revolves round the Sun.

The time of the revolution of Venus round the Sun may be deduced from the interval which elapses between two or more conjunctions, by help of the following theorem:

110. Let two bodies, A and B, revolve uniformly in the same direction, and let a and b be their respective periods, of which b is the least, and t the interval between two successive conjunctions or oppositions.

Then $b=\frac{a\,t}{a+t}$, and $a=\frac{b\,t}{t-b}$.

For the angular motions are inversely proportional to the periodic times. Therefore the angular motions of A and B are as $\frac{1}{a}$ and $\frac{1}{b}$. And, since they move in the same direction, the synodical or relative motion is the difference of their angular motions. Therefore the fundamental equation is $\frac{1}{b}-\frac{1}{a}=\frac{1}{t}$. Hence $\frac{1}{b}=\frac{1}{t}+\frac{1}{a}$, $=\frac{a+t}{a\,t}$, and $b=\frac{a\,t}{a+t}$. Also $\frac{1}{a}=\frac{1}{b}-\frac{1}{t}$, $=\frac{t-b}{t\,b}$, and $a=\frac{b\,t}{t-b}$.

We may also calculate the synodical period t, when we know the real periods of each. For $\frac{1}{t}=\frac{1}{b}-\frac{1}{a}=\frac{a-b}{a\,b}$, and $t=\frac{a\,b}{a-b}$.

This gives, for the periodic time of Venus round the Sun, 224^d 16^h 49′ 13″.

111. But it is evident, that if this angular motion is not uniform, the interval between two successive conjunctions may chance to give a false measure of the period. But by observing many conjunctions in various parts of the heavens, and by dividing the interval between the first and last by the number of intervals between each (taking care that the first and last shall be nearly in the same part of the heavens), it is evident that the inequalities being distributed among them all, the quotient may be taken as nearly an exact medium. Hence arises the great value of ancient observations. In eight years we have five conjunctions of Venus, and she is only 1° 32′ short of the place of the first conjunction. The period deduced from the conjunctions in 1761 and 1769, scarcely differs from that deduced from the conjunctions in 1639 and 1761. But the other planets require more distant observations.

112. Venus does not move uniformly in her orbit. For if the place of Venus in the heavens be observed in a great number of successive conjunctions with the Sun (at which time her place in the ecliptic, as seen from the Sun, is either the Sun's place, as seen from the Earth, or the opposite to it), we find that her changes of place are not proportional to the elapsed times. By observations of this kind, we learn the inequality of the angular motion of Venus round the Sun, and hence can find the equations for every point of the orbit of Venus, and can thence deduce the position of Venus, as seen from the Sun, for any given instant.

This, however, requires more observations of this kind than we are yet possessed of, because her conjunctions happen so nearly in the same points of her orbit, that great part of it is left without observations of this kind. But we have other observations of almost equal value, namely, those of her greatest elongations from the Sun. There is none of the planets, therefore, of which the equations (which indeed are very small) are more accurately determined.

113. We can now determine the form and position of the orbit. For we can *observe* the place of the Sun, or the position of the line E S, (Fig. 13.) joining the Earth and Sun. We know the length of this line (53.) We can *observe* the GEOCENTRIC place of Venus, or the position of the line E D joining the Earth and Venus. And we can compute (112.) the HELIOCENTRIC place of Venus, or the position of the line S C joining Venus and the Sun. Venus must be in V, the intersection of these two lines; and therefore that point of her orbit is determined.

114. By such observations Kepler discovered, that the orbit of Venus is an ellipse, having the Sun in one focus, the semitransverse axis being 72333, and the eccentricity 510, measured on a scale of which the Sun's mean distance from the Earth is 100000.

115. The upper apsis of the orbit is called the APHELION, and the lower apsis is called the PERIHELION of Venus.

116. The line of the apsides has a slow motion eastward, at the rate of 2° 44′ 46″ in a century.

117. The orbit of Venus is inclined to the ecliptic at an angle of 3° 20′, and the nodes move westward about 31″ in a year.

118. Venus moves in this orbit so as to describe round the Sun areas proportional to the times.

119. The planet Mercury resembles Venus in all the circumstances of her apparent motion; and we make similar inferences with respect to the real motions. His orbit is discovered to be an ellipse, having the Sun in one focus.

The semitransverse axis is 38710, and the eccentricity 7960. The apsides move eastward 1° 57′ 20″ in a century. The orbit is inclined to the ecliptic 7°. The nodes move westward 45″ in a year. The periodic time is $87^d\ 23^h\ 15'\ 37''$, and areas are described proportional to the times.

Of the proper Motions of the Superior Planets.

120. Mars, Jupiter, and Saturn, exhibit phenomena considerably different from those exhibited by Mercury and Venus.

1. They come to our meridian both at noon and at midnight. When they come to our meridian at noon, and are in the ecliptic, they are never seen crossing the Sun's disk. Hence we infer, that their orbits include both the Sun and the Earth.

2. They are always retrograde when in opposition, and direct when in conjunction.

The planet Jupiter may serve as an example of the way in which their real motions may be investigated.

121. Jupiter is an opaque body, visible by means of the reflected light of the Sun. For the shadows of some of the heavenly bodies are sometimes observed on his disk, and his shadow frequently falls on them.

122. His apparent diameter, when in opposition, is about 46″, and, when in conjunction, it is about 31″, and his disk is always round. Hence we infer, that he is nearest when in opposition, and that his least and greatest distance are nearly as two to three. The Earth is, therefore, far removed from the centre of his motion; and, if we endeavour to explain his motion by means of a deferent circle and an epicycle, the radius of the deferent must be about five times the radius of the epicycle.

123. Since Jupiter is always retrograde when in opposition, and direct when in conjunction, his position, with respect to the centre of his epicycle, must be similar to the

position of the Sun with respect to the Earth. His motion, therefore, in the epicycle, has a dependance on the motion of the Sun; and his motion, as seen from the Sun, must be simpler than as seen from the Earth.

His position, as seen from the Sun, may be accurately *observed* in every opposition and conjunction.

It was very natural for the ancient astronomers of Greece to infer, from what has been said just now, that the position of Jupiter, in respect of the centre of his epicycle, was the same as that of the Sun in respect of the Earth, not only in opposition and conjunction, but in every other situation. For, in twelve years, we see it to be so in the oppositions observed in twelve parts of the heavens, and in 83 years we see it in 76 parts. It is very improbable, therefore, that it should be otherwise in the intervals.

The motion of a superior planet may be explained upon these principles in the following manner:

Let T (Fig. 12.) be the Earth, and α β κ δ ι φ γ χ α be the Sun's orbit. Also, let A, B, C, D, E, F, G, H, I, be the places of the centre of the epicycle in the circumference of the deferent when the Sun is in α, β, κ, δ, ι, φ, γ, χ, α, make A *a* parallel to T α, and B *b* parallel to T β, and C *c* parallel to T κ, &c. and make these lines of a length that is duly proportioned (by the Apollonian rule) to the radius T A of the deferent circle.

When the Sun is in α, β. κ, &c. the centre of the epicycle is in A, B, C, &c. and the planet is in *a*, *b*, *c*, &c. and the dotted curve *a b c d e f g h a k* is its path in absolute space between two succeeding oppositions to the Sun, viz. in *a*, and in *k*.

124. If we make the radius of Jupiter's deferent circle to that of the epicycle, as 52 to 10, the epicyclical motion arising from this construction will very nearly agree with the observation. Only we may observe, that the oppositions which succeed each other near the constellation Virgo, are less distant from one another than those observed in the

opposite part of the heavens; so that the centre of the epicycle seems to move slower in the first case than in the last. To reconcile this with the perfect uniformity of the motion of that centre in the circumference of the deferent circle, the ancient astronomers said that the earth was not exactly in the centre of the deferent, but so placed that the equable motion of the centre of the epicycle appeared slower, because it is then more remote; and after various trials, they fixed on a degree of eccentricity for the deferent, which accorded better than any other with the observations, and really differed very little from them. Copernicus shews that their hypothesis for Jupiter never deviates more than half a degree from observation, if it be properly employed. They found that the epicycle moved round the deferent in 4332⅓ days, with an equation gradually increasing to near 6 degrees; so that if the place of the epicycle be calculated for a quarter of a revolution from the apogee, at the mean rate of 5′ per day, it will be found too far advanced by near ten weeks motion.

125. But the ancient astronomers had no such data for determining the absolute magnitude of the deferent circles and epicycles for the superior planets, as Mercury and Venus afforded them. The rules given them by Apollonius only taught them what proportion the epicycle of each planet must have to its deferent circle, but gave no information as to the absolute magnitude of either, or the proportion between the deferent circles of any two superior planets. Accordingly, no two ancient astronomers agree in their measures, farther than in saying that Saturn is farther off than Jupiter, and Jupiter than Mars. This they inferred from their longer periods. All they had to take care of was to make their sizes sufficiently different, so that the epicycles of two neighbouring planets should not cross and justle each other. Yet they might easily have come very near the truth, by a small and very allowable addition to their hypothesis of epicyclical motion, namely, by suppos-

ing that the epicycle of each planet is equal to the Sun's orbit. This was quite allowable.

126. If we do this, we shall deduce consequences that are very remarkable, and which would have put the ancient astronomy on a footing very near to perfection. For, if C *c* (Fig. 12.) be not only parallel to T γ, but also equal to it, then C T γ *c* is a parallelogram, and γ *c* is equal and parallel to T C. The bearing (to express it as a mariner) and distance of Jupiter from the Sun is at all times the same with the bearing and distance of the centre of his epicycle from the Earth; and Jupiter is always found in an orbit round the Sun, equal and similar to the deferent orbit round the Earth. Thus, α *a* is equal to T A; β *b* to T B; γ *c* to T C, &c. with respect to all the points of the looped curve. If the Earth be in the centre of the deferent, the distance of Jupiter from the Sun is always the same, and he may be said to describe a circle round the Sun, while the Sun moves round the Earth. Nay, it results from the equality of A *a* to T α of B *b* to T β, &c. that whatever eccentricity, or whatever form it has been thought necessary to assign to the deferent, the distances α *a*, β *b*, γ *c*, &c. will still be respectively equal to T A, T B, T C, &c. The circle which the astronomers called the deferent, because it is supposed to carry Jupiter's epicycle round the Earth, may be supposed to accompany the Sun, being carried round by him in a year, the line of its apsides (124.) keeping parallel to itself, that is, in our figure, to T A. And thus, the motion of Jupiter round the Sun will be incomparably more simple than the looped curve round the Earth; for it will be precisely the motion which was given by the astronomers to the centre of Jupiter's epicycle. The motion of Jupiter in absolute space is indeed the same looped curve in both cases; but the way of conceiving it is much much more simple.

127. This supposition of the equality of Jupiter's epicycle to the Sun's orbit, and the parallelism of C *c* to T γ

in every position of Jupiter, are fully verified by the modern discoveries of his satellites. These little planets revolve round him with perfect regularity, their shadows frequently fall on his disk, and they are often obscured by his shadow. This shews the position of Jupiter's shadow at all times, and, consequently, Jupiter's position in respect of the Sun. This we find at all times to be parallel to the supposed position of the centre of his epicycle. Thus *s c* is found parallel to T C.

128. We now can tell the precise point in which Jupiter is found in any moment of time. Having made the radius T *s* to the radius T A in the due proportion of 10 to 52, and having placed the Earth at the proper distance from the centre of the deferent Q A L, we can calculate (60.) the position and length of the line T *s* joining the Earth with the Sun. We can draw the line T C to the supposed centre of Jupiter's epicycle, having learned the law or equation of the supposed motion of that centre by our observation oppositions in all quarters of the ecliptic (124.) and we then draw *s* V parallel to it. This must pass through Jupiter, or Jupiter must be somewhere in this line. We observe Jupiter, however, in the direction T Z. Jupiter must therefore be in the intersection *c* of the lines *s* V and T Z. And then we can measure *c s*, Jupiter's distance from the Sun.

129. Kepler, by taking this method with a series of observations made by Tycho Brahé, discovered that Jupiter was always found in the circumference of an ellipse, having the Sun in its focus. Its semitransverse axis is 520098, the mean distance of the Earth from the Sun being supposed 100000. Its eccentricity is 25277. Its inclination to the ecliptic is 1° 20′, and the nodes move eastward about 1′ in a year.

130. The revolution in this orbit is completed in 4332½ days, and areas are described proportional to the times.

131. Proceeding in the same manner, we discover that

the planets Mars, Saturn, and the one discovered by Dr Herschel in 1781, are always found in the circumference of ellipses, with the Sun in one focus, and describe round him areas proportional to the times.

The chief circumstances of their motions are stated as follows:

	Mean Distance.	*Eccentricity.*	*Period in Days.*
Georgian planet	1908584	90738	30456,07
Saturn - - - -	953941	53210	10759,27
Mars - - - -	152369	14218	686,98

132. Two other bodies have lately been detected in the planetary regions, revolving round the Sun in orbits which do not seem very eccentric, and seem placed between those of Mars and Jupiter. The first was observed in 1801 by Mr Piazzi of Palermo, and by him named Ceres. The other was discovered in 1802 by Mr Olbers of Bremen, who has called it Pallas. They are exceedingly small, and we have seen too little of their motions as yet to enable us to state their elements with any precision.*

133. Thus it has been discovered, that, while the Sun revolves round the Earth, the six planets now mentioned are always found in the circumferences of ellipses, having the Sun in one focus, and that they describe round the Sun areas proportional to the times.

134. But now, instead of supposing that the centre of a small epicyle is carried round the circumference of a greater deferent circle, different for each planet, we may rather consider the Sun's orbit round the Earth as the only deferent circle, and suppose that the planets describe their

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* The following are the chief circumstances of their motions; the mean distance of the Earth being 100,000.

	Mean distance.	Eccentricity.	Period in Days.		
Pallas, -	279100	24630	1703^d	17^h	Sider. Revol.
Ceres, -	276500	8141	1681	12	Tropical ditto.
Juno, - -	265700	25096	1588		Ditto.
Vesta, -	237300	9322	1155		Ditto.

ED.

great elliptical epicycles round him with different periods, while he moves round the Earth in a year. The real motions of the planets are still the same looped curves in both cases. For, in either case, the motion of a planet is compounded of the same motions. But the latter supposition is much more probable. We can scarcely conceive the motion of Jupiter in the epicycle *q r s* as having any physical relation to its centre, a mere mathematical point of space. We cannot consider this point as having any physical properties that shall influence the motions of the planet. This point also is supposed to be in motion, carrying with it the influence by which the planet is retained in the circumference of the epicycle. This is another inconceivable circumstance. This combination of circles, therefore, cannot be considered as any thing but a mere mathematical hypothesis, to furnish some means of calculation, or for the delineation of the looped path of the planet. Accordingly, the first proposers of these epicycles, sensible of the mere nothingness of their centre, and the impossibility of a nothing moving in the circumference of a circle, and drawing a planet along with it, farther supposed that the epicycles were vast solid transparent globes, and that the planet was a luminous point or star, sticking in the surface of this globe. And, to complete the hypothesis, they supposed that the globe turned round its centre, carrying the planet round with it, and thus produced the direct and retrograde motions that we observe. Aristotle taught, that this motion was effected by the genius of the planet residing in the globe, and directing it, as the mind of man directs his motions. But, further, to account for the motion of this globe in the circumference of the deferent, the ancient philosophers supposed, that the deferent was also a vast crystalline, or, at least, transparent material spherical shell, turning round the earth, and that this shell was of sufficient thickness to receive the epicyclic globe within its solid substance, not adhering, but at liberty to turn round its own centre. This hypothesis, though more like the dream of a feverish man than the thoughts of one in his senses, was received as unquestionable, from the time of Aristotle till that of Copernicus. It is scarcely credible, that thinking men should admit its truth for a minute, even in its most admissible form. But as the art of observing improved, it was found necessary to add another epicycle to the one already admitted, in order to account for an annual inequality in the epicyclical motion. This was a small transparent globe, placed where Aristotle placed the planet, and the planet was stuck on *its* surface. Even this was found insufficient, and another set of epicycles were added, till, in short, the heavens were filled with solid matter. It is needless to say any more of this epicyclical doctrine and machinery.

135. But the other mode of conceiving the planetary motions, while it equally furnishes the means of calculation or graphical operation, has much more the appearance of reality. The Sun's motion is round the Earth, which we are naturally disposed to think the centre of the world; and the planets revolve, not round a mathematical point, a nothing, but round the Sun, a real and very remarkable substance.

136. Kepler, to whom we are indebted for this discovery of the elliptical motions, and the equable description of areas, also observed, that the squares of the periodic times in these ellipses are proportional to the cubes of the mean distances from the Sun. He also observed the same analogy with respect to the Sun's period and distance from the Earth.

137. The distances here alluded to are all taken from a scale of equal parts, of which the Sun's mean distance from the Earth, contains 100000. But astronomers wish to know the absolute quantity of those distances in some known measures. This may be learned by means of the parallax of any one of the planets. Thus, let Mars be in M, (Fig. 14.) and let his distance from some fixed star C be observed by two persons on the surface of the Earth at

A and B. The difference G D of the observed distances C G, C D, will give the angle D M G, or its equal A M B. The angles M A B and M B A are given by observation, and the line A B is given; and therefore A M, and consequently E M, may be computed in miles.

The transit of Venus across the Sun's disk affords much better observations for this purpose. For, at the time, Venus is much nearer to the Earth than Mars is when in opposition, their distances from us being nearly as 28 to 52. Therefore the distance between the observers will subtend a larger angle at Venus. This may be measured by the distance between the apparent tracks of Venus across the Sun's disk. A spectator in Lapland, for example, sees Venus move in the line C D, (Fig. 15) while one at the Cape of Good Hope sees her move in the line A B. Also, as C D is a shorter chord than A B, the transit will occupy less time. This difference in time, amounting, in some fortunate cases, to many minutes, will give a very exact measure of the interval between those two chords.

138. The transits in 1761 and 1769 were employed for this purpose, at the earnest recommendation of Dr Edmund Halley. From those observations, combined with the proportions deduced from Kepler's third law, we may assume the following distances from the Sun in English statute miles, as pretty near the truth.

The Earth - -	93,726,900
Mercury - - -	36,281,700
Venus - - -	67,795,500
Mars - - - -	142,818,000
Jupiter - - -	487,472,000
Saturn - - -	894,162,000
Georgian Planet - -	1,789,982,000*

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* The following are the distances of the new planets, in miles;

	Miles.		Miles.
Pallas	265,000,000	Juno -	254,000,000
Ceres -	263,000,000	Vesta -	225,000,000 Ed.

Of the Secondary Planets.

139. Jupiter is observed to be always accompanied by four small planets called SATELLITES, which revolve round him, while he revolves round the Sun.

Their distances from Jupiter are measured by means of their greatest elongations, and their periods are discovered by their eclipses, when they come into his shadow, and by other methods. They are observed to describe ellipses, having Jupiter in one focus; and they describe areas round Jupiter, which are proportional to the times. Also the squares of their periods are in the proportion of the cubes of their mean distances from Jupiter.

140. It has been discovered, by means of the eclipses of Jupiter's satellites, that light is propagated in time, and employs about 8′ 11″ in moving along a line equal to the mean distance of the Earth from the Sun.

The times of the revolutions of these little bodies had been studied with the greatest care, on account of the easy and accurate means which their frequent eclipses gave us for ascertaining the longitudes of places. But it was found, that, after having calculated the time of an eclipse in conformity to the periods, which had been most accurately determined, the eclipse happened later than the calculation, in proportion as Jupiter was farther from the Earth. If an eclipse, when Jupiter is in opposition, be observed to happen precisely at the time calculated, an eclipse three months before, or after, when Jupiter is in quadrature, will be observed to happen about eight minutes later than the calculated time. An eclipse happening about six weeks before or after opposition, will be about four minutes later than the calculation, when those about the time of Jupiter's opposition happen at the exact time. In general, this *retardation* of the eclipses is observed to be exactly proportional to the increase of Jupiter's distance from the Earth. It is the same with respect to all the satellites. This error greatly perplexed the astronomers, till the connexion of it with Jupiter's change of distance was remarked by Mr Roemer, a Danish astronomer, in 1674. As soon as this gentleman took notice of this connexion, he concluded that the retardation of the eclipse was owing to the time employed by the light in coming to us. The satellite, now eclipsed, continued to be seen, till the *last* reflected light reached us, and, when the stream of light ceased, the satellite disappeared, or was eclipsed. When it has passed through the shadow, and is again illuminated, it is not seen at that instant by a spectator almost four hundred millions of miles off—it does not reappear to him till the *first* reflected light reaches him. It is not till about forty minutes after being re-illuminated by the Sun, that the first reflected light from the satellite reaches the Earth when Jupiter is in quadrature, and about thirty-two minutes when he is in opposition.

This ingenious inference of Mr Roemer was doubted for some time, but most of the eminent philosophers agreed with him. It became more probable, as the motions of the satellites were more accurately defined; and it received complete confirmation by Dr Bradley discovering another, and very different consequence of the progressive motion of light from the fixed stars and planets. This will be considered afterwards; and, in the mean time, it is evinced that light, or the cause of vision, is propagated in time, and requires about 16½ minutes to move along the diameter of the Sun's orbit, or about 8′ 11″ to come from the Sun to us, moving about 200,000 miles in a second. Some imagine vision to be produced by the undulation of an elastic medium, as sound is produced by the undulation of air. Others imagine light to be emitted from the luminous body, as a stream of water from the disperser of a watering-pan. Whichever of these be the case, light now becomes a proper subject of mechanical discussion; and we may now speculate about its motions, and the forces which produce and regulate them.

141. Saturn is also observed to be accompanied by seven satellites, which circulate round him in ellipses, having Saturn in the focus. They describe areas proportional to the times, and the squares of the periodic times are proportional to the cubes of their mean distances.

142. Besides this numerous band of satellites, Saturn is also accompanied by a vast arch or ring of coherent matter, which surrounds him, at a great distance. Its diameter is about 208,000 miles, and its breadth about 40,000. It is flat, and extremely thin; and as it shines only by reflecting the Sun's light, we do not see it when its edge is turned towards us. Late observation has shewn it to be two rings, in the same plane, and almost united. But that they are separated, is demonstrated by a star being seen through the interval between them. Its plane makes an angle of 29° or 30° with that of Saturn's orbit; and when Saturn is in 11^{s} 20°, or 5^{s} 20°, the plane of the ring passes through the Sun, and reflects no light to us.

143. In 1787, Dr Herschel discovered two satellites attending the Georgian planet; and in 1798, he discovered four more. Their distances and their periodic times observe the laws of Kepler; but the position of their orbits is peculiarly interesting. Instead of revolving in the order of the signs, in planes not deviating far from the ecliptic, their orbits are almost, if not precisely perpendicular to it; so that it cannot be said that they move either in the order of the signs, or in the opposite.

144. Thus do they present a new problem in physical astronomy, in order to ascertain the Sun's influence on their motions—the intersection of their nodes, and the other disturbances of their motions round the planet.

145. They also shew the mistake of the cosmogonists, who would willingly ascribe the general tendency of the planetary motions from west to east along the ecliptic to

the influence of some general mechanical impulsion, instructing us how the world may be made as we see it. These perpendicular orbits are incompatible with the supposed influence.

Of the Rotation of the Heavenly Bodies.

146. In 1611, Scheiner, professor at Ingolstadt, observed spots on the disk of the Sun, which come into view on the eastern limb, move across his disk in parallel circles, disappear on the western limb, and, after some time, again appear on the eastern limb, and repeat the same motions. Hence it is inferred that the Sun revolves from west to east in the space of $25^d\ 14^h\ 12'$, round an axis inclined to the plane of the ecliptic 7½ degrees, and having the ascending node of his equator in longitude $2^s\ 10^\circ$.

Philosophers have formed various opinions concerning the nature of these spots. The most probable is, that the Sun consists of a dark nucleus, surrounded by a luminous covering, and that the nucleus is sometimes laid bare in particular places. For the general appearance of a spot during its revolution is like Fig. 15.

147. A series of most interesting observations has been lately made by Dr Herschel, by the help of his great telescopes. These observations are recorded in the Philosophical Transactions for the years 1801 and 1802. They lead to very curious conclusions respecting the peculiar constitution of the Sun. It would seem that the Sun is immediately surrounded by an atmosphere, heavy and transparent, like our air. This reaches to the height of several thousand miles. On this atmosphere seems to float a stratum of shining clouds, also some thousands of miles in thickness. It is not clear, however, that this cloudy stratum shines by its native light. There is above it, at some distance, another stratum of matter, of most dazzling splendour. It would seem that it is this alone which illumi-

nates the whole planetary system, and also the clouds below it. This resplendent stratum is not equally so, but most luminous in irregular lines or ridges, which cover the whole disk like a very close brilliant network. Something of this appearance was noticed by Mr James Short, in 1748, while observing a total eclipse of the Sun, and is mentioned in the Philosophical Transactions. Some operation of nature in this solar atmosphere seems to produce an upward motion in it, like a blast, which causes both the clouds and the dazzling stratum to remove from the spot, making a sort of hole in the luminous strata, so that we can see through them, down to the dark nucleus of the Sun. Dr Herschel has observed, that this change, and this denudation of the nucleus, is much more frequent in some particular places of the Sun's disk. He has also observed a small bit of shining cloud come in at one side of an opening, and, in a short time, move across it, and disappear on the other side of the opening; and he thinks that these moving clouds are considerably below the great cloudy stratum.

148. Dr Herschel is disposed to think, that the upper resplendent stratum never shines on the nucleus, not even when an opening has been made in the stratum of clouds. For he remarks, that the upper stratum is always much more driven aside by what produces the opening than the clouds are; so that even the most oblique rays from the splendid stratum do not go through, being intercepted by the border of clouds which immediately surround the opening.

149. From Dr Herschel's description of this wonderful object, we are almost led to believe that the surface of the Sun may not be scorched with intolerable and destructive heat. It not unfrequently happens, that we have very cold weather in summer, when the sky is overcast with thick clouds, impenetrable by the direct rays of the Sun. The curious observations of Count Rumford of the man-

ner in which heat is most copiously communicated through fluid substances, concur with what we knew before, to shew us, that even an intense heat, communicated by radiation to the upper surface of the shining clouds by the dazzling stratum above them, may never reach far down through their thickness. With much more confidence may we affirm, that it would never warm the transparent atmosphere below those clouds, nor scorch the firm surface of the Sun. It is far from being improbable, therefore, that the surface may not be uninhabitable, even by creatures like ourselves. If so, there is presented to our view a scene of habitation 13,000 times bigger than the surface of this Earth, and about 50 times greater than those of all the planets added together.

150. Similar observations, first made by Dr Hooke, in 1664, on spots in the disk of Jupiter, show that he revolves from west to east in 9ʰ 56′, round an axis inclined to the plane of his orbit 2½°. It is also observed, that his equatoreal diameter is to his axis nearly as 14 to 13.

151. There are some remarkable circumstances in the rotation of this planet. The spots, by whose change of place on the disk we judge of the rotation, are not permanent, any more than those observed on the Sun's disk. We must therefore conclude, that either the surface of the planet is subject to very considerable variations of brightness, or that Jupiter is surrounded by a cloudy atmosphere. The last is, of itself, the most probable, and it becomes still more so from another circumstance. There is a certain part of the planet that is sensibly brighter than the rest, and sometimes remarkably so. It is known to be one and the same part by its situation. This spot turns round in somewhat less time than the rest. That is, if a dark spot remains during several revolutions, it is found to have separated a little from this bright spot, to the left hand, that is, to the westward. There is a minute or two of difference between the rotation of Jupiter, as deduced from the suc-

cessive appearances of the bright spot, and that deduced from observations made on the others.

152. These circumstances lead us to imagine, that Jupiter is really covered with a cloudy atmosphere, and that this has a slow motion from east to west relative to the surface of the planet. The striped appearances, called Belts or Zones, are undoubtedly the effect of a difference of climate. They are disposed with a certain regularity, generally occupying a complete round of his surface. Mr Schroeter, who has minutely studied their appearances for a long tract of time, and with excellent glasses, says, that the changes in the atmosphere are very anomalous, and often very sudden and extensive; in short, there seems almost the same unsettled weather as on this globe. He does not imagine that we ever see the real surface of Jupiter; and even the bright spot which so firmly maintains its situation, is thought by Schroeter to be in the atmosphere. The general current of the clouds is from east to west, like our trade-winds, but they often move in other directions. The motion is also frequently too rapid to be thought the transference of an individual substance; it more resembles the rapid propagation of some short-lived change in the state of the atmosphere, as we often observe in a thunder storm. The axis of rotation is almost perpendicular to the plane of the orbit, so that the days and nights are always equal.

153. The rotation of Mars, first observed by Hooke and Cassini in 1666, is still more remarkable than that of Jupiter. The surface of the planet is generally of unequal brightness, and something like a permanent figure may be observed in it, by which we guess at the time of the rotation. But the figure is so ill defined, and so subject to considerable changes, that it was long before astronomers could be certain of a rotation, so as to ascertain the time. Dr Herschel has been at much pains to do this with accuracy, and, by comparing many successive apparitions of

the same objects, he has found that the time of a revolution is 24 hours and 40 minutes, round an axis inclined to the plane of the ecliptic in an angle of nearly 60 degrees, but making an angle of 61° 18′ with his own orbit.

154. It is midsummer-day in Mars when he is in long. $11^s\ 19^\circ$ from our vernal equinox. As the planet is of a very oblate form, and probably hollow, there may be a considerable precession of his equinoctial points, by a change in the direction of his axis.

155. Being so much inclined to the ecliptic, the poles of Mars come into sight in the course of a revolution. When either pole comes first into view, it is observed to be remarkably brighter than the rest of the disk. This brightness gradually diminishes, and is generally altogether gone, before this pole goes out of sight by the change of the planet's position. The other pole now comes into view, and exhibits similar appearances.

156. This appearance of Mars greatly resembles what our own globe will exhibit to a spectator placed on Venus or Mercury. The snows in the colder climates diminish during summer, and are renewed in the ensuing winter. The appearances in Mars may either be owing to snows, or to dense clouds, which condense on his circumpolar regions during his winter, and are dissipated in summer. Dr Herschel remarks, that the atmosphere of Mars extends to a very sensible distance from his disk.

157. Observers are not agreed as to the time of the rotation of Venus. Some think that she turns round her axis in 23^h, and others make it 23 days and 8 hours. The uncertainty is owing to the very small time allowed for observation, Venus never being seen for more than three hours at a time, so that the change of appearance that we observe, day after day, may either be a *part* of a slow rotation, or more than a complete rotation made in a short time. Indeed no distinct spots have been observed in her disk since the time of the elder Cassini, about the middle

of the seventeenth century. Dr Herschel has always observed her covered with an impenetrable cloud, as white as snow, and without any variety of appearance.

158. The Moon turns round her axis in the course of a periodic month, so that one face is always presented to our view. There is indeed a very small LIBRATION, as it is called, by which we occasionally see a little variation, so that the spot which occupies the very centre of the disk, when the Moon is in apogee and in perigee, shifts a little to one side, and a little up or down. This arises from the perfect uniformity of her rotation, and the unequal motion in her orbit. As the greatest equation of her orbital motion amounts to little more than 5°, this causes the central spot to shift about $\frac{1}{24}$ of her diameter to one side, and, returning again to the centre, to shift as far to the other side. She turns always the same face to the other focus of her elliptical orbit round the Earth, because her angular motion round that point is almost perfectly equable.

159. It has been discovered by Dr Herschel, that Saturn turns round his axis in $10^h\ 16'$, and that his ring turns round the same axis in $10^h\ 32\frac{1}{4}'$. This axis is inclined to the ecliptic in an angle of 60° nearly, and the intersection of the ring and ecliptic is in the line passing through long. $5^s\ 20°$ and $11^s\ 20°$. We see it very open when Saturn is in long. $2^s\ 20°$, or $8^s\ 20°$; and its length is then double of its apparent breadth. It is then midsummer and midwinter on Saturn. When Saturn is in the line of its nodes, it disappears, because its plane passes through the Sun, and its edge is too thin to be visible. It shines only by reflecting the Sun's light. For we sometimes see the shadow of Saturn on it, and sometimes its shadow on Saturn. It will be very open in 1811. Just now (1803) it is extremely slender, and it disappeared for a while in the month of June. Its diameter is above 200,000 miles, almost half of that of the Moon's orbit round the Earth.

160. No rotation can be observed in Mercury, on account of his apparent minuteness,[*] nor is any observed in the Georgian planet for the same reason.

161. Many philosophers have imagined, that the Earth revolves round its axis in 23ʰ 56′ 4″ from west to east; and that this is the cause of the observed diurnal motion of the heavens, which is therefore only an appearance. It must be acknowledged, that the appearances will be the same, and that we must be insensible of the motion. There are also many circumstances which render this rotation very probable.

162. 1. All the celestial motions will be rendered incomparably more moderate and simple. If the heavens really turn round the Earth in 23ʰ 56′ 4″, the motion of the Sun, or of any of the planets, is swifter than any motion of which we have any measure, and this to a degeee almost beyond conception. The motion of the Sun would be *20,000* times swifter than that of a cannon ball. That of the Georgian planet will be twenty times greater than this. If the Earth turns round its axis, the swiftest motion necessary for the appearances is that of the Earth's equator, which does not exceed that of a cannon ball.

The motions also become incomparably simpler. For the combination of diurnal motion with the proper motion of the planets makes it vastly more complex, and impossible to account for on any mechanical principles. This diurnal motion must vary, in all the planets, by their change of declination, being about $\frac{1}{6}$ slower when they are near the tropics. Yet we cannot conceive that any physical relation can subsist between the orbital motion of a planet and the position of the Earth's equator, sufficient for producing such a change in the planet's motion. Besides, the axis of diurnal revolution is far from being the same just now and in the time of Hipparchus. Just now, it passes near the star in the extremity of the tail of the Little Bear. When Hipparchus observed the heavens, it passed near the snout of the Camelopard. It is to the last degree improbable, that every object in the universe has changed its motion in this manner. It must be supposed that all have changed their motions in different degrees, yet all in a certain precise order, without any connexion or mutual dependance that we can conceive.

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[*] The diurnal rotation of Mercury has been found to be performed in 24ʰ 5′ 28″. Juno is conjectured to revolve in about 27 hours.

ED.

163. 2. There is no withholding the belief, that the Sun was intended to be a source of light and genial warmth to the organized beings which occupy the surface of our globe. How much more simply, easily, and beautifully, this is effected by the Earth's rotation, and how much more agreeably to the known economy of nature!

164. 3. This rotation would be analogous to what is observed in the Sun and most of the planets.

165. 4. We observe phenomena on our globe that are necessary consequences of rotation, but cannot be accounted for without it. We know that the equatoreal regions are about twenty miles higher than the circumpolar; yet the waters of the ocean do not quit this elevation, and retire and inundate the poles. This may be prevented by a proper degree of rotation. It may be so swift, that the waters would all flow toward the equator, and inundate the torrid zone; nay, so swift, that every thing loose would be thrown off, as we see the water dispersed from a twirled mop. Now, a very simple calculation will shew us, that a rotation in $23^h\ 56'$ is precisely what will balance the tendency of the waters to flow from the elevated equator towards the poles, and will keep it uniformly spread over the whole spheroid. We also observe, that a lump of matter of any kind weighs more (by a spring steelyard) at Spitzbergen than at Quito, and that the diminution of gravity is precisely what would arise from the supposed rotation, viz. $\frac{1}{289}$.

There are arguments which give the most convincing demonstration of the Earth's rotation.

166. 1. Did the heavens turn round the Earth, as has long been believed, it is almost certain that no zodiacal fixed star could be seen by us. For it is highly probable, that light is an emission of matter from the luminous body. If this be the case, such is the distance of any fixed star A, (Fig. 16.) that, when its velocity A C is compounded with the velocity of light emitted in any direction A B, or A *b*, it would produce a motion in a direction A D, or A *d*, which would never reach the Earth, or which might chance to reach it, but with a velocity infinitely below the known velocity of light; and, in *any* hypothesis concerning the nature of light, the velocity of the light by which we see the circumpolar stars, must greatly exceed that by which we see the equatoreal stars. All this is contrary to observation.

2. The shadow of Jupiter, also, should deviate greatly from the line drawn from the Sun to Jupiter, just as we see a ship's vane deviate from the direction of the wind, when she is sailing briskly across that direction. If the diurnal revolution is a real motion, when Jupiter is in opposition, his first satellite must be seen to come from behind his disk, and, after appearing for about 1^h 10′, must be eclipsed. This is also contrary to observation; for the satellites are eclipsed precisely when they come into that line, whereas it should happen more than an hour after.

167. We must therefore conclude, that the Earth revolves round its axis from west to east in 23^h 56′ 4″. We must further conclude, from the agreement of the ancient and modern latitudes of places, that the axis of the Earth is the same as formerly; but that it changes its position, as we observe in a top whose motion is nearly spent. This change of position is seen by the shifting of the equinoctial points. As these make a tour of the ecliptic in 25972 years, the pole of the equator, keeping always per-

pendicular to its plane, must describe a circle round the pole of the ecliptic, distant from it 23^d 28′, the inclination of the equator to the ecliptic. It will be seen, in due time, that this motion of the Earth's axis, which appeared a mystery even to Copernicus, Tycho Brahé and Kepler, is a necessary consequence of the general power of nature by which the whole assemblage is held together; and the detection of this consequence is the most illustrious specimen of the sagacity of the discoverer, Sir Isaac Newton.

Of the Solar System.

168. We have seen (134.) that the planets are always found in the circumferences of ellipses, which have the Sun in their common focus, while the Sun moves in an ellipse round the Earth. The motion of any planet is compounded of any motion which it has in respect of the Sun, and any motion which the Sun has in respect of the Earth. Therefore (92 93.) the appearances of the planetary motions will be the same as we have described, if we suppose the Sun to be at rest, and give the Earth a motion round the Sun, equal and opposite to what the Sun has been thought to have round the Earth.

In the second part of that article concerning relative motion, it was shewn that the relative motion, or change of motion, of the body B, as seen from A, is equal and opposite to that of A seen from B. In the present case, the distance of the Sun from the Earth is equal to that of the Earth from the Sun. The position or bearing is the opposite. When the Earth is in Aries or Taurus, the Sun will be seen in Libra or Scorpio. When the Earth is in the tropic of Capricorn, the Sun will appear in that of Cancer, and her north pole will be turned towards the Sun; so that the northern hemisphere will have longer days than nights. In short, the gradual variation of the seasons will be the same in both cases, if the Earth's axis keeps the same position during its revolution round the Sun. It must do so, if there be no force to change its position; and we see that the axis of the other planets retain their position.

169. Then, with respect to the planets, the appearances of direct and retrograde motion, with points of station, will also be the same as if the Sun revolved round the Earth. That this may be more evident, it must be observed, that our judgment of a planet's situation is precisely similar to that of a mariner who sees a ship's light in a dark night. He sets it by the compass. If he sees it due north, and a few minutes after, sees it a little to the westward of north, he imagines that the ship has really gone a little westward. Yet this might have happened, had both been sailing due east, provided that the ship of the spectator had been sailing faster. It is just the same in the planetary motions. If we give the Earth the motion that was ascribed to the Sun, the real velocity of the Earth will be more than double of the velocity of Jupiter Now suppose, according to the old hypothesis, the Earth at T (Fig. 12) and the Sun at α. Suppose Jupiter in opposition. Then we must place the centre of his epicycle in A, and make A a equal to T α. Jupiter is in a, and his bearing and distance from the Earth is T a, nearly 4-5ths of T A. Six weeks after, the Sun is in β; the centre of Jupiter's epicycle is in B. Draw B b equal and parallel to T β, and b is now the place of Jupiter, and T b is now his bearing and distance. He has changed his bearing to the right hand, or westward on the ecliptic; and his change of position is had by measuring the angle a T b. His longitude on the ecliptic is diminished by this number of degrees.

170. Now let the Sun be at T, according to the new hypothesis, and let A B E L be Jupiter's orbit round the Sun. Let Jupiter be in opposition to the Sun. We must place Jupiter in A, and the Earth in ι, so as to have the Sun and Jupiter in opposition. It is evident that Jupiter's bearing and distance from the Earth are the same as in the

former hypothesis. For A *a* being equal to ε T we have ε A, the distance of Jupiter from the Earth, equal to T *a* of the former hypothesis. Six weeks after, the Earth is at φ, and Jupiter at B. Join φ B, and draw φ N parallel to T A. It is evident that the distance φ B of Jupiter from the Earth, is equal to the distance T *b* of the former construction. Also the angle N φ B, which is Jupiter's change of bearing, (by the astronomer's compass, the ecliptic), is equal to the angle *a* T *b* of the former construction. Jupiter therefore, instead of moving to the left hand, has moved to the right, or westward, and has diminished his ecliptical bearing or longitude by the degrees in the angle N′ φ B.

171. In the same manner may the apparent motion of Jupiter be ascertained for every situation of the Earth and Jupiter; and it will be found that, in every case, the line corresponding to φ B is equal and parallel to the line corresponding to T *b*; thus γ C is equal and parallel to T *c*; χ D is equal and parallel to T *d*, &c.

The apparent motions of the planets are therefore precisely the same in either hypothesis, so that we are left to follow either opinion, as it appears best supported by other arguments.

172. Accordingly, it has been the opinion of some philosophers, both in ancient and modern times, that the Earth is a planet, revolving round the Sun placed in the focus of its elliptical orbit, and that it is accompanied by the Moon, in the same manner as Jupiter and Saturn are by their satellites.

The following are the reasons for preferring this opinion to that contained in the 133d and 135th articles, which equally explains all the phenomena hitherto mentioned, and is more consistent with our first judgments.

173. 1. The celestial motions become incomparably more simple, and free of those looped contortions which must be supposed in the other case, and which are extremely

improbable, and incompatible with what we know of the laws of motion.

174. 2. This opinion is also more reasonable, on account of the extreme minuteness of the Earth, when compared with the immense bulk of the Sun, Jupiter, and Saturn; and because the Sun is the source of light and heat to all the planets.

The reasons adduced in this and the preceding article were all that could offer themselves to the philosophers of antiquity. They had not the telescope, and the satellites were therefore unknown. They had no knowledge of the powers of nature by which the planetary motions are produced and regulated; their knowledge of dynamical science was extremely scanty. Yet Pythagoras, Philolaus, Apollonius, Anaxagoras, and others, maintained this opinion. But they had few followers in an opinion so different from our habitual thoughts, and for which they could only offer some reasons founded on certain notions of propriety or suitableness. But, as men became more conversant, in modern times, with the mechanical arts, every thing connected with the motion of bodies became more familiar, and was better understood, and we had less hesitation in adopting sentiments unlike the first and most familiar suggestions of sense. Other arguments now offered themselves.

175. 3. If the Earth turns round the Sun, then the analogy between the squares of the periodic times and the cubes of the distances, will obtain in all the bodies which circulate round a common centre; whereas this will not be the case with respect to the Sun and Moon, if both turn round the Earth.

176. 4. It is thought that the motion of the Sun round the Earth is inconsistent with the discoveries which have been made concerning the forces which operate in the planetary motions.

We have seen, by an article in dynamics, combined with the third law of motion, that neither can the Sun revolve round the Earth at rest, nor the Earth round the Sun at rest, but that both must revolve round their common centre of position. It is discovered that the quantity of matter in the Sun is more than 300,000 times that of the matter in the Earth. Therefore the centre of position of these two bodies must be almost in the centre of the Sun. Nay, if all the planets were on one side of the Sun, the common centre would be very near his centre.

177. But, perhaps, this argument is not of the great weight that is supposed. The discovery of the proportion of these quantities of matter seems to depend on its being previously established that the Sun is in, or near, the centre of position of the whole assemblage. It must be owned, however, that the perfect harmony of all the comparative measures of the quantities of matter of the Sun and planets, deduced from sources independent of each other, renders their accuracy almost unquestionable.

178. 5. It is incontestably proved by observation. A motion has been discovered in all the fixed stars, which arises from a combination of the motion of light with the motion of the Earth in its orbit.

Suppose a shower of hail falling during a perfect calm, and therefore falling perpendicularly. Were it required to hold a long tube in such a position that a hailstone shall fall through it without touching either side, it is plain that the tube must be held perpendicular. Suppose now that the tube is fastened to the arm of a gin, such as those employed in raising coals from the pit, and that it is carried round, with a velocity that is equal to that of the falling hail. It is now evident that a perpendicular tube will not do. The hailstones will all strike on the hindmost side of the tube. The tube must be put into the direction of the *relative* motion of the hailstones. Now, it was demonstrated that this is the diagonal of a parallelogram, one side of which is the real motion of the hail, and the other is equal, but opposite, to the motion of the tube. Therefore, if the tube be inclined *forward*, at an angle of 45°, the experiment will succeed, because the tangent of this angle is equal to the radius; and, while the hailstone falls two feet, the tube advances two, and the hailstone will pass along the tube without touching it.

In the very same manner, if the Earth be at rest, and we would view a star near the pole of the ecliptic, the telescope must be pointed directly at the star. But if the Earth be in motion round the Sun, the telescope must be pointed a little forward, that the light may come along the axis of the tube. The proportion of the velocity of light to the supposed velocity of the Earth in its orbit is nearly that of 10,000 to 1. Therefore the telescope must lean about 20″ forward.

Half a year after this, let the same star be viewed again. The telescope must again be pointed 20″ a-head of the true position of the star: but this is in the opposite direction to the former deviation of the telescope, because the Earth, being now in the opposite part of its orbit, is moving the other way. Therefore the position of the star must appear to have changed 40″ in the six months.

It is easy to shew that the consequence of this is, that every star must appear to have 40″ more longitude when it is on our meridian at night, than when it is on the meridian at mid-day. The effect of this composition of motions, which is most susceptible of accurate examination, is the following. Let the declination of some star near the pole of the ecliptic be observed at the time of the equinoxes. It will be found to have 40″ more declination in the autumnal than in the vernal equinox, if the observer be in latitude 60° 30′; and not much less if he be in the latitude of London. Also every star in the heavens should appear to describe a little ellipse, whose longer axis is 40″.

179. Now this is actually observed, and was discovered by Dr Bradley about the year 1726. It is called the ABER-

RATION OF THE FIXED STARS, and is one of the most curious and most important discoveries of the eighteenth century. It is important, by furnishing an incontrovertible proof that the Earth is a planet, revolving, like the others, round the Sun. It is also important, by shewing that the light of the fixed stars moves with the same velocity with the light of the Sun, which illuminates our system.

180. This arrangement of the planets is called the COPERNICAN SYSTEM, having been revived and established by Copernicus, represented in Fig. A. The other opinion, mentioned (133), which equally explains the general phenomena, was maintained by Longomontanus.

181. Account of the PTOLEMAIC, EGYPTIAN, and TYCHONIC systems (Fig. B, C, D.)

182. The Copernican system is now universally admitted; and it is fully established, 1. That the planets and the comets describe round the Sun areas proportional to the times; and that the Moon, and the satellites of Jupiter and Saturn, describe round the Earth, Jupiter, and Saturn, areas proportional to the times. 2. That the orbits described by those bodies are ellipses, having the Sun, or the primary planet, in one focus. 3. That the squares of the periodic times of these bodies which revolve round a common centre are proportional to the cubes of their mean distances from that centre. These three propositions are called the LAWS OF KEPLER.

183. There is however an objection to this account of the planetary motions, which has been thought formidable. Suppose a telescope pointed in a direction perpendicular to the plane of the Earth's orbit, and carried round the Sun in this position. Its axis, produced to the starry firmament, should trace out a figure precisely equal and similar to the orbit, and we should be able to mark it among the stars round the pole of the ecliptic. But, if this be tried, we find that we are always looking at the same point, which

always remains the centre of the little ellipse which is the effect of the aberration of light.

This objection was made, even in the schools of Greece, to Aristarchus of Samos, when he used his utmost endeavours to bring into credit the later opinion of Pythagoras, placing the Sun in the centre of the system, And the answer given by Aristarchus is the only one that we can give at the present day.

184. The only answer that can be given to this is, that the distance of the fixed stars is so great, that a figure of near 200 millions of miles diameter is not a sensible object. This, incredible as it may seem, has nothing in it of absurdity. We know that their distance is immense. The comet of 1680 goes 150 times farther from the Sun than we are, and we must suppose it much farther from the nearest star, that it may not be affected by it in its motion round our Sun. Suppose it only twice as far, the Earth's orbit traced among the stars would appear only half the diameter of the Sun. We have telescopes which magnify the diameter of objects 1200 times. Yet a fixed star is not magnified by them in the smallest degree. That is, though we were only at the 1200dth part of our present distance from it, it would appear no bigger. The more perfect the telescope is, the stars appear the smaller. We need not be surprised, therefore, that observation shews no parallax of the fixed stars, not even 1″. Yet a parallax of 1″ puts the object 206,000 times farther off than the Sun. But space is without bounds, and we have no reason to think that our view comprehends the whole creation. On the contrary, it is more probable that we see but an inconsiderable part of the scene on which the perfections of the Creator and Governor of the universe are displayed.

Of the Comets.

185. There are sometimes seen in the heavens certain bodies, accompanied by a train of faint light, which has occasioned them to be called comets. Their appearance and motions are extremely various; and the only general remarks that can be made on them are, that the train, or tail, is generally small on the first appearance of a comet, gradually lengthens as the comet comes into the neighbourhood of the Sun, and again diminishes as it retires to a distance. Also the tail is always extended in a direction nearly opposite to the Sun.

186. The opinions of philosophers concerning comets have been very different. Sir Isaac Newton first showed that they are a part of the solar system, revolving round the Sun in trajectories, nearly parabolical, having the Sun in the focus. Dr Halley computed the motions of several comets, and, among them, found some which had precisely the same trajectory. He therefore concluded, that these were different appearances of one comet, and that the path of a comet is a very eccentric ellipse, having the Sun in one focus. The apparition of the comet of 1682 in 1759, which was predicted by Halley, has given his opinion the most complete confirmation.

187. Comets are therefore planets, resembling the others in the laws of their motion, revolving round the Sun in ellipses, describing areas proportional to the times, and having the squares of their periodic times proportional to the cubes of their mean distances from the Sun. They differ from the planets in the great variety in the position of their orbits, and in this, that many of them have their course *in antecedentia signorum*.

188. Their number is very great; but there are but few with the elements of whose motions we are well acquainted. The comet of 1680 came very near to the Sun on the 11th of December, its distance not exceeding his

semidiameter. When in its aphelion, it will be almost 150 times farther from the Sun than the Earth is. Our ideas of the extent of the solar system are thus greatly enlarged.

189. No satisfactory knowledge has been acquired concerning the cause of that train of light which accompanies the comets. Some philosophers imagine that it is the rarer atmosphere of the comet, impelled by the Sun's rays. Others imagine, that it is the atmosphere of the comet, rising in the solar atmosphere by its specific levity. Others imagine, that it is a phenomenon of the same kind with the aurora borealis, and that this Earth would appear like a comet to a spectator placed on another planet. Consult Newton's *Principia*;—a Dissertation, by Professor Hamilton of Trinity College, Dublin;—a Dissertation by Mr Winthorpe of New Jersey, &c.; both in the *Philosophical Transactions*.

PHYSICAL ASTRONOMY

190. It is hoped, that the preceding account of the celestial phenomena has given the attentive student a distinct conception of the nature of that evidence which Kepler had for the truth of the three general facts discovered by him in all the motions, and for the truth of those seeming deviations from Kepler's laws, which were so happily reconciled with them by Sir Isaac Newton, by shewing, that these deviations are examples of mutual deflections of the celestial bodies towards one another. Several phenomena were occasionally noticed, although not immediately subservient to this purpose. These are the chief objects of our subsequent attempts to explain. The account given of the kind of observation, by which the different motions were proved to be what has been affirmed of them, has been exceedingly short and slight, on the presumption that the young astronomer will study the celestial phenomenology in the detail, as delivered by Gregory, Keill, and other authors of reputation. This study will terminate in the fullest conviction of the validity of the evidence for the truth of the Copernican system of the Sun and planets; and in a minute acquaintance with all those peculiarities of motion that distinguish the individuals of the magnificent assemblage.

We are now in a condition to investigate the particular characters of those extensive powers of nature, those mechanical affections of matter, which cause the observed deviations from that uniform rectilineal motion which would have been observed in every body, had it been under no mechanical influence. And we shall also be able to explain or account for the distinguishing peculiarities of motion which characterize the individuals of the system, if we shall so far succeed in our first investigation, as to shew that no other force operates in the system, and that these peculiarities are only particular and accurately narrated cases of the three general laws, precisely conformable to their legitimate consequences.

In our first investigation, we must affirm the forces to be such as are indicated by the motions, in the manner agreed on in the general doctrines of Dynamics. That is, the kind and the intensity of the force must be inferred from the direction and the magnitude of the change which we consider as its effect.

In all this process, it is plain, that we consider the heavenly bodies as consisting of matter that has the same mechanical properties with the bodies which are daily in our hands. We are not at liberty to imagine, that the celestial matter has any other properties than what is indicated by the motions, otherwise we have no explanation, and may as well rest contented with the simple narration of the facts. The constant practice, in all attempts to explain a natural appearance, is to try to find a class of familiar phenomena which resemble it; and if we succeed, we account it to be one of the number, and we rest satisfied with this as a sufficient explanation. Accordingly, this is the way that philosophers, both in ancient and modern times, have proceeded in their attempt to discover the causes of the planetary motions.

191. 1. Nothing is more familiar to our experience than bodies carried round fixed centres by means of solid matter connecting the bodies with the centre, in one way or another. This was the first attempt to explain the planetary motions of which we have any account. Eudoxus and Callippus, many ages before our era, taught, that all the stars in the firmament are so many lucid points or bodies, adhering to the inside of a vast material concave sphere, which turned round the Earth placed in the centre in twenty-four hours. It was called the CRYSTALLINE ORB or Sphere.

But this will not explain the easterly motion of the Sun and Moon, unless we suppose them endowed with some self-moving power, by which they can creep slowly eastward along the surface of the crystalline orb; far less will it account for the Moon sometimes hiding the Sun from us. These philosophers were therefore obliged to say, that there were other spheres, or rather spherical shells, transparent, like vast glass globes, one within another, and all having a common centre. The Sun and the Moon were supposed to be attached to the surface of those globes. The sphere which carried the Moon was the smallest, immediately surrounding the Earth. The sphere of the Sun was much larger, but still left a vast space between it and the sphere of the fixed stars, which contained all.

This machinery may make a shift to carry round the Moon, the Sun, and the stars, in a way somewhat like what we behold. But the planets gave the philosophers much trouble, in order to explain their retrograde and direct motions, and stationary points, &c. To move Jupiter in a way resembling what we behold, they supposed the shell of his sphere to be of vast thickness, and in its solid matter they lodged a small transparent sphere, in the surface of which Jupiter was fixed. This sphere turned round in the hollow made for it in the thick shell of the deferent sphere, and, as all was transparent, exhibited Jupiter moving to the westward, when his episphere brought him toward us, and to the east, when it carried him round toward the outer surface of the deferent shell. Meanwhile, the great deferent globe was moving slowly eastward, or rather was turning more slowly westward, than the sphere of the stars.

No doubt, this mechanism will produce round-about motions, and stations, and retrogradations, &c. This, however, is only a very gross outline of the planetary motions. But the Sun's unequable motion could not be represented without supposing the Earth out of the centre of rotation of his sphere. This was accordingly supposed, and it was an easy supposition. But the motion of Jupiter, in relation to the centre of his epicycle, must be similar to the Sun's motion in relation to the Earth (123.); but a solid sphere, turning in a hollow which exactly fits it, can only turn round its centre. This is evident. Therefore the inequality of Jupiter's epicyclical motion cannot be represented by this mechanism. The deferent sphere may be eccentric, but the epicycle cannot. This obliged those engineers to give Jupiter a secondary epicycle much smaller than the epicycle which produced his retrogradations and stations. It moved in a hollow lodgement made for it in the solid matter of the epicyle, just as this moved in a hollow in the solid matter of the deferent globe.

Even this would not correspond, with tolerable exactness, with the observed tenor of Jupiter's motion; other epicycles were added, to tally with every improvement made on the equation of the apparent motion, till the whole space was almost crammed full of solid matter; and after all these efforts, some mathematicians affirmed, that there are motions in the heavens that are neither uniform nor circular, nor can be compounded of such motions. If so, this spherical machinery is impossible. In modern times, Tycho Brahé proved, beyond all contradiction, that the comet of 1574 passed through all those spheres, and therefore their existence was a mere fiction.

One should think the whole of this contrivance so artless and rude, that we wonder that it ever obtained the least credit; yet was it adopted by the prince of ancient philosophers—by Aristotle; and his authority gave it possession of all the schools till modern times.

But where, all this while, is the mover of all this machinery? Aristotle taught, that each globe was conducted, or turned round its axis, by a peculiar genius or demon. This was worthy of the rest; and when such assertions are called *explanations*, nothing in nature need remain unexplained. We must, however, do Hipparchus and Ptolemy the justice to say, that they never adopted this hypothesis of Eudoxus and Callippus; they did not speculate about the causes, but only endeavoured to ascertain the motions; and their epicycle and deferent circles are given by them merely as steps of mathematical contemplation, and in order to have some principle to direct their calculation, just as we demonstrate the parabolic path of a cannon ball by compounding a uniform motion in the line of direction with a uniformly accelerated motion in the vertical line. There is no such composition, but the motion of the ball is the same as if there were.

192. 2. A much more feasible attempt was made by Cleanthes, another philosopher of Greece, to assign the causes of the planetary motions. He observed, that bodies are easily carried round in whirlpools or vortices of water. He taught, that the celestial spaces are filled with an ethereal fluid, which is in continual motion round the Earth, and that it carried the Sun and planets round with it. But a slight examination of this specious hypothesis shewed, that it was much more difficult to form a notion of the vortices, so as to correspond with the observed motions, than to study the motions themselves. It therefore gave no explanation. Yet this very hypothesis was revived in modern times, and was maintained by two of the most eminent mathematicians and philosophers of Europe, namely, by Des Cartes and Leibnitz; and, for a long while, it was acquiesced in by all.

We must constantly keep in mind, that an explanation always means to shew that the subject in question is an example of something that we clearly understand. Whatever is the avowed property of that more familiar subject, must therefore be admitted in the use made of it for explanation. We explain the splitting of glass by heat, by shewing, that the known and avowed effects of heat make the glass swell on one side to a certain degree, with a certain known force; and we shew, that the tenacity of the other side of the glass, which is not swelled by the heat, is not able to resist this force which is pulling it asunder; it must therefore give way. In short, we shew the splitting to be one of the ordinary effects of heat, which operates here as it operates in all other cases.

Now, if we take this method, we find, that the effects of a vortex or whirl in a fluid are totally unlike the planetary motions, and that we cannot ascribe them to the vortical motion of the ether, without giving it laws of motion unlike every thing observed in all the fluids that we know; nay, in contradiction of all those laws of mechanics which are admitted by the very patrons of the hypothesis. To give this fluid properties unknown in all others, is absurd; we had better give those properties to the planets themselves. The fact is, that these two philosophers had not taken the trouble to think about the matter, or to inquire what motions of a vortex of fluid are possible, and what are not, or what effects will be produced by such vortices as are possible. They had not thought of any means of moving the fluid itself, or for preserving it in motion; they contented themselves (at least this was the case with Des Cartes) with merely throwing out the general fact, that bodies *may* be carried round by a vortex. It is to Sir Isaac Newton that we are indebted for all that we know of vortical motion. In examining this hypothesis of Des Cartes, which had supreme authority among the philosophers at that time, he found it necessary to inquire into the manner in which a vortex may be produced, and the constitution of the vortex which results from the mode of its production. This led him, by necessary steps, to discover what forms of vortical motion are possible, what are permanent, and the variations to which the others are subject. In the second book of his Mathematical Principles of Natural Philosophy, he has given the result of this examination; and it contains a beautiful system of mechanical doctrine, concerning the mutual action of the filaments of fluid matter, by which they modify each other's motion. The result of the whole was a complete refutation of this hypothesis as an explanation of the planetary motions, shewing that the legitimate consequences of a vortical motion are altogether unlike the planetary motions, nay, are incompatible with them. It is quite enough, in this place, for proving the insufficiency of the hypothesis, to observe, that it must explain the motion of the comets as well as that of the planets. If Mars be carried round the Sun by a fluid vortex, so is the comet which appeared in 1682 and 1759. This comet came from an immense distance, in the northern quarter of the heavens, into our neighbourhood, passing through the vortices of all the planets, describing its very eccentric ellipse with the most perfect regularity. Now, it is absolutely impossible, that, in one and the same place, there can be passing a stream of the vortex of a planet, and a stream of the cometary vortex, having a direction and a velocity so very different. It is inconceivable that these two streams of fluid shall have force enough, one of them to drag a planet along with it, and the other to drag a comet, and yet that the particles of the one stream shall not disturb the motion of those of the other in the smallest degree; even the infinitely rare vapour which formed the tail of the comet was not in the least deranged by the motion of the planetary vortices through which it passed. All this is inconceivable and absurd.

It is a pity that the account given by Newton of vortical motions appeared on such an occasion; for this limited the attention of his readers to this particular employment of it, which purpose being completely answered in another way, this argument became unnecessary, and was not looked into. But it contains much valuable information, of great service in all problems of hydraulics. Many consequences of the mutual action of the fluid filaments produce important changes on the motion of the whole; so that till these are understood and taken into the account, we cannot give an answer to very simple, yet important questions. This is the cause why this branch of mechanical philosophy is in so imperfect a state, although it is one of the most important.

193. 3. Many of the ancient philosophers, struck with the order, regularity, and harmonious co-operation of the planetary motions, imagined that they were conducted by intelligent minds. Aristotle's way of conceiving this has been already mentioned. The same doctrine has been revived, in some respect, in modern times. Leibnitz animates every particle of matter, when he gives his *Monads* a perception of their situation with respect to every other monad, and a motion in consequence of this perception. This, and the elemental mind ascribed by Lord Monboddo to every thing that begins motion, do not seem to differ much from the *ἴστρως ψυχη* of Aristotle; nor do they differ from what all the world distinguishes by the name of *force*.

This doctrine cannot be called a hypothesis; it is rather a definition, or a misnomer, giving the name Mind to what exhibits none of those phenomena by which we distinguish mind. No end beneficial to the agent is gained by the motion of the planet. It may be beneficial to its inhabitants—But should we think more highly of the mind of an animal when it is covered with vermin?—Nor does this doctrine give the smallest explanation of the planetary motions. We must explain the motions by studying them, in order to discover the laws by which the action of their cause is regulated: this is just the way that we learn the nature of any mechanical force. Accordingly,

194. 4. Many philosophers, both in ancient and modern times, imagined that the planets were deflected from uniform rectilineal motion by forces similar to what we observe in the motions of magnetical and electrical bodies, or in the motion of common heavy bodies, where one body seems to

influence the motion of another at a distance from it, without any intervening impulsion. It is thus that a stone is bent continually from the line of its direction towards the Earth. In the same manner, an iron ball, rolling along a level table, will be turned aside toward a magnet, and, by properly adjusting the distance and the velocity, the ball may be made to revolve round the pole of the magnet. Many of the ancients said that the curvilineal motions of the planets were produced by *tendencies* to one another, or to a common centre. Among the moderns, Fermat is the first who said in precise terms that the weight of a body is the sum of the tendencies of each particle to every particle of the Earth. Kepler said still more expressly, that if there be supposed two bodies, placed out of the reach of all external forces, and at perfect liberty to move, they would approach each other, with velocities inversely proportional to their quantities of matter. The Moon (says he) and the Earth mutually attract each other, and are prevented from meeting by their revolution round their common centre of attraction. And he says that the tides of the ocean are the effects of the Moon's attraction, heaping up the waters immediately under her. Then, adopting the opinion of our countryman, Dr Gilbert of Colchester, that the Earth is a great magnet, he explains how this mutual attraction will produce a deflection into a curvilineal path, and adds, '*Veritatis in me fit amor an gloria, loquantur*
'*dogmata mea, quæ pleraque ab aliis accepta fero. Totam*
'*astronomiam Copernici hypothesibus de mundo, Tychonis*
'*vero Brahei observationibus, denique Gulielmi Gilberti*
'*Angli philosophiæ magneticæ inædifico.*'

EPIT. ASTR. COPERN.

195. The most express surmise to this purpose is that of Dr Robert Hooke, one of the most ardent and ingenious students of nature in that busy period. At a meeting of the Royal Society, on May 3, 1666, he expressed himself in the following manner.

'I will explain a system of the world very different from 'any yet received; and it is founded on the three follow'ing positions.

'1. That all the heavenly bodies have not only a gra'vitation of their parts to their own proper centre, but that 'they also mutually attract each other within their spheres 'of action.

'2. That all bodies having a simple motion, will con'tinue to move in a straight line, unless continually deflect'ed from it by some extraneous force, causing them to 'describe a circle, an ellipse, or some other curve.

'3. That this attraction is so much the greater as the 'bodies are nearer. As to the proportion in which those 'forces diminish by an increase of distance, I own (says 'he) I have not discovered it, although I have made some 'experiments to this purpose. I leave this to others, who 'have time and knowledge sufficient for the task.'

This is a very precise enunciation of a proper philosophical theory. The phenomenon, the change of motion, is considered as the mark and measure of a changing force, and his audience is referred to experience for the nature of this force. He had before this exhibited to the Society a very pretty experiment contrived on these principles. A ball, suspended by a long thread from the ceiling, was made to swing round another ball laid on a table immediately below the point of suspension. When the push given to the pendulum was nicely adjusted to its deviation from the perpendicular, it described a perfect circle round the ball on the table. But when the push was very great, or very small, it described an ellipse, having the other ball in its centre. Hooke shewed that this was the operation of a deflecting force proportional to the distance from another ball. He added, that although this illustrated the planetary motions in some degree, yet it was not suitable to their cause. For the planets describe ellipses having the Sun, not in the centre, but in the focus. Therefore they are not retained by a force proportional to their distance from the Sun. This was strict reasoning, from good principles. It is worthy of remark, that in this clear, and candid, and modest exposition of a rational theory, he anticipated the discoveries of Newton, as he anticipated, with equal distinctness and precision, the discoveries of Lavoisier, a philosopher inferior perhaps only to Newton.

Thus we see that many had noticed certain points of resemblance between the celestial motions and the motions of magnets and heavy bodies. But these observers let the remark remain barren in their hands, because they had neither examined with sufficient attention the celestial motions, which they attempted to explain, nor had they formed to themselves any precise notions of the motions from which they hoped to derive an explanation.

196. At last a genius arose, fully qualified, both by talents and disposition, for those arduous tasks. I speak of Sir Isaac Newton. This ornament, this boast of our nature, had a most acute and penetrating mind, accompanied by the soundest judgment, with a modest and proper diffidence in his own understanding. He had a patience in investigation, which I believe is yet without an equal, and was convinced that this was the only compensation attainable for the imperfection of human understanding, and that when exercised in prosecuting the conjectures of a curious mind, it would not fail of giving him all the information that we are warranted to hope for. Although only 24 years of age, Mr Newton had already given the most illustrious specimen of his ability to promote the knowledge of nature, in his curious discoveries concerning light and colours. These were the result of the most unwearied patience, in making experiments of the most delicate kind, and the most acute penetration in separating the resulting phenomena from each other, and the clearest and most precise logic in reasoning from them; and they terminated in forming a body of science which gave a total change to all the notions of

philosophers on this subject. Yet this body of optical science was nothing but a fair narration of the facts presented to his view. Not a single supposition or conjecture is to be found in it, nor reasoning on any thing not immediately before the eye; and all its science consisted in the judicious classification. This had brought to light certain general laws, which comprehended all the rest. Young Newton saw that this was sure ground, and that a theory, so founded, could never be shaken. He was determined therefore to proceed in no other way in all his future speculations, well knowing that the fair exhibition of a law of nature is a discovery, and all the discovery to which our limited powers will ever admit us. For he felt in its full force the importance of that maxim so warmly inculcated by Lord Bacon, that nothing is to be received as proved in the study of nature that is not logically inferred from an observed fact; that accurate observation of phenomena must precede all theory; and that the only admissible theory is a proof that the phenomena under consideration is included in some general fact, or law of nature.

197. Retired to his country house, to escape the plague which then raged at Cambridge where he studied, and one day walking in his garden, his thoughts were turned to the causes of the planetary motions. A conjecture to this purpose occurred to him. Adhering to the Baconian maxim, he immediately compared it with the phenomena by calculation. But he was misled by a false estimation he had made of the bulk of the Earth. His calculation shewed him that his conjecture did not agree with the phenomenon. Newton gave it up without hesitation; yet the difference was only about a sixth or seventh part; and the conjecture, had it been confirmed by the calculation, was such as would have acquired him great celebrity. What youth but Newton could have resisted such a temptation? But he thought no more of it.

As he admired Des Cartes as the first mathematician of

Europe, and as his desire of understanding the planetary motions never quitted his mind, he set himself to examine, in his own strict manner, the Cartesian theory, which at this time was supreme in the universities of Europe. He discovered its nullity, but would never have published a refutation, hating controversy above all things, and being already made unhappy by the contests to which his optical discoveries had given occasion. His optical discoveries had recommended him to the Royal Society, and he was now a member. There he learned the accurate measurement of the Earth by Picard, differing very much from the estimation by which he had made his calculation in 1666; and he thought his conjecture now more likely to be just. He went home, took out his old papers, and resumed his calculations. As they drew to a close, he was so much agitated, that he was obliged to desire a friend to finish them. His former conjecture was now found to agree with the phenomena with the utmost precision. No wonder then that his mind was agitated. He saw the revolution he was to make in the opinions of men, and that he was to stand at the head of philosophers.

198. Newton now saw a grand scene laid open before him; and he was prepared for exploring it in the completest manner; for, ere this time, he had invented a species of geometry that seemed precisely made for this research. Dr Hooke's discourse to the Society, and his shewing that the pendulum was not a proper representation of the planetary forces, was a sort of challenge to him to find out that law of deflection which Hooke owned himself unable to discover. He therefore set himself seriously to work on the great problem, to 'determine the motion of a body under 'the continual influence of a deflecting force.' There were found among his papers many experiments on the force of magnets; but this does not seem to have detained him long. He began to consider the motions of terrestrial bodies with an attention that never had been bestowed on them before;

and in a short time composed twelve propositions, which contained the leading points of celestial mechanism. Some years after, viz. in 1683, he communicated them to the Royal Society, and they were entered on record. But so little was Newton disposed to court fame, that he never thought of publishing, till Dr Edmund Halley, the most eminent mathematician and philosopher in the kingdom, went to visit him at Cambridge, and never ceased importuning and entreating him, till he was prevailed on to bring his whole thoughts on the subject together, digested into a regular system of universal mechanics. Dr Halley was even obliged to correct the manuscript, to get the figures engraved, and, finally, to take charge of the printing and publication. Newton employed but eighteen months to compose this immortal work. It was published at last, in 1687, under the title of *Mathematical Principles of Natural Philosophy*, and will be accounted the sacred oracles of natural philosophy as long as any knowledge remains in Europe.

199. It is plain, that in this process of investigation, in order to explain the planetary motions by means of our knowledge of motions that are more familiar, Newton was obliged to suppose that the planets consist of common matter, in which we infer the nature of the moving cause from the motions that we observe. Newton's first step, therefore, was a scrupulous observation of the celestial motions, knowing that any mistake with regard to these must bring with it a similar mistake with regard to the natural power inferred from it. Every force, and every degree of it, is merely a philosophical interpretation of some change of motion according to the Copernican system. The Earth is said to gravitate towards the Sun, because, and only because, she describes a curve line concave toward the Sun, and areas proportional to the times. If this be not true, it is not true that the Earth gravitates to the Sun. For this reason, a doubt was expressed (177.), whe-

ther the Newtonian discoveries were used with propriety as arguments for the truth of the Copernican system.

Most fortunately for science, the real motions of the heavenly bodies had been at last detected; and the sagacious Kepler had reduced them all to three general facts, known by the name of the laws of Kepler.

200. The first of those laws is, that *all the planets move round the Sun in such a manner that the line drawn from a planet to the Sun passes over or describes* (verrit, *sweeps*) *areas proportional to the times of the motion.*

Hence Newton made his first and great inference, *that the deflection of each planet is the action of a force always directed toward the Sun* (219.), that is, such, that if the planet were stopped, and then let go, it would move toward the Sun in a straight line, with a motion continually accelerated, just as we observe a stone fall toward the Earth. Subsequent observation has shewn this observation to be much more extensive than Kepler had any notion of; for it comprehends above ninety comets, which have been accurately observed. A similar action or force is observed to connect the Moon with this Earth, four satellites with Jupiter, seven with Saturn, and six with Herschel's planet, all of which describe round the central body areas proportional to the times. Newton ascribed all these deflections to the action of a mechanical force, on the very same authority with which we ascribe the deflection of a bombshell, or of a stone, from the line of projection to its *weight*, which all mankind consider as a *force*. He therefore said that *the primary planets are retained in their paths round the Sun, and the satellites in their paths round their respective primaries, by a force tending toward the central body.* But it must be noticed that this expression ascertains nothing but the direction of this force, but gives no hint as to its manner of acting. It may be the impulse of a stream of fluid moving toward that centre; or it may be the attraction of the central body. It may be a tendency inherent in the planet—it may be the influence of some ministering spirit—but, whatever it is, this is the direction of its effect.

201. Having made this great step, by which the relation of the planets to the Sun is established, and the Sun proved to be the great regulator of their motions, Newton proceeded to inquire farther into the nature of this deflecting force, of which nature he had discovered only one circumstance. He now endeavoured to discover what variation is made in this deflection by a change of distance. If this follow any regular law, it will be a material point ascertained. This can be discovered only by comparing the momentary deflections of a planet in its different distances from the Sun. The magnitude or intensity of the force must be conceived as precisely proportional to the magnitude of the deflection which it produces in the same time, just as we measure the force of terrestrial gravity by the deflection of sixteen feet in a second; which we observe, whether it be a bombshell flying three miles, or a pebble thrown to the distance of a few yards, or a stone simply dropped from the hand. Hence we infer, that gravity is every where the same. We must reason in the same way concerning the planetary deflections in the different parts of their orbits.

Kepler's second law, with the assistance of the first, enabled Newton to make this comparison. This second general fact is, that *each planet describes an ellipse, having the Sun in one focus.* Therefore, to learn the proportion of the momentary deflections in different points of the ellipse, we have only to know the proportion of the arches described in equal small moments of time. This we may learn by drawing a pair of lines from the Sun to different parts of the ellipse, so that each pair of lines shall comprehend equal areas. The arches on which these areas stand must be described in equal times; and the proportion of their linear deflections from the tangents must be taken for the proportion of the deflecting forces which produced them.

To make those equal areas, we must know the precise form of the ellipse, and we must know the geometrical properties of this figure, that we may know the proportion of those linear deflections.

202. *The force by which a planet describes areas proportional to the times round the focus of its elliptical orbit is, as the square of its distance from the focus inversely.*

Let F be the deflecting force in the aphelion A (Fig. 17.) and f the force in any intermediate point P. Let V and v be the velocities in A and P, and C and c be the deflective chords of the equicurve circles in those points.

Then, by a dynamical proposition we have $F : f = \frac{V^2}{C} : \frac{v^2}{c}$, or $= V^2 c : v^2 C$. But, when areas are described proportional to the times, the velocity in A is to that in P inversely as the perpendiculars drawn from F to the tangents in A and P. F A is perpendicular to the tangent in A, and F N is perpendicular to the tangent P N. Therefore $F : f = \frac{c}{FA^2} : \frac{C}{FN^2} = FN^2 \times c : FA^2 \times C$.

But it is known that C, the deflective chord at A, is equal to L the principal parameter of the ellipse. It is also known that P O is half the deflective chord at P, and that P R is half the principal parameter L. Moreover, the triangles F N P and P Q O and P Q R are similar, and therefore $FN : FP = PQ : PO$. But $PO : PQ = PQ : PR$. Therefore $PO : PR = PO^2 : PQ^2$. Therefore $FN^2 : FP^2 = PR : PO$, and $FN^2 + PO = FP^2 \times PR$, and $FN^2 \times 2\,PO = FP^2 \times PR$; that is, $FN^2 \times c = FP^2 \times L$.

Therefore $F : f = FP^2 \times L : FA^2 \times L, = FP^2 : FA^2$, that is, inversely as the square of the distance from F.

203. This proposition may be demonstrated more briefly, and perhaps more palpably, as follows:

If P p be a very minute arch, and $p\,r$ be perpendicular to the radius vector P F, then $q\,p$, the linear deflection from the tangent is, ultimately, in the proportion of $p\,r^2$. But, because equal areas are described in equal times, the elementary triangle P F p is a constant quantity, when the moments are supposed equal, and therefore $p\,r$ is inversely as P F, and $p\,r^2$ inversely as P F^2. Therefore $q\,p$ is inversely as P F^2 or the momentary deflection from the tangent is inversely as the square of P F, the distance from the focus. Now, the momentary deflection is the measure of the deflecting force, and the force is inversely as the square of the distance from the focus.

Here, then, is exhibited all that we know of that property or mechanical affection of the masses of matter which compose the solar system. Each is under the continual influence of a force directed toward the Sun, urging the planet in that direction; and this force is variable in its intensity, being more intense as the planet comes nearer to the Sun; and this change is in the inverse duplicate ratio of its distance from the Sun. It will free us entirely from many metaphysical objections which have been made to this inference, if, instead of saying that the planets manifest such a variable tendency toward the Sun, we content ourselves with simply affirming the fact, that the planets are continually deflected toward the Sun, and that the momentary deflections are in the inverse duplicate ratio of the distances from him.

204. We must affirm the same thing of the forces which retain the satellites in their elliptical orbits round their primary planets. For they also describe ellipses having the primary planet in the focus; and we must also include the Halleyan comet, which shewed, by its re-apparition in 1759, that it describes an ellipse having the Sun in the focus. If the other comets be also carried round in eccentric ellipses, we must draw the same conclusion. Nay, should they describe parabolas or hyperbolas having the Sun in the focus, we should still find that they are retained by a force in-

versely proportional to the square of the distance. This is demonstrated in precisely the same manner as in the case of elliptical motion; namely, by comparing the linear deflections corresponding to equal elementary sectors of the parabola or hyperbola. These are described in equal times, and the linear deflections are proper measures of the deflecting forces. We shall find in both of those curves $q p$ proportional to $p r^2$. It is the common property of the conic sections referred to a focus.

It is most probable that the comets describe very eccentric ellipses. But we get sight of them only when they come near to the Sun, within the orbit of Saturn. None has yet been observed as far off as that planet. The visible portion of their orbits sensibly coincides with a parabola or hyperbola, having the same focus; and their motion, computed on this supposition, agrees with observation. The computation in the parabola is very easy, and can then be transferred to an ellipse by an ingenious theorem of Dr Halley's in his *Astronomy of Comets*. M. Lambert of Berlin has greatly simplified the whole process. The student will find much valuable information on this subject in M'Laurin's *Treatise of Fluxions*. The chapters on curvature and its variations, are scarcely distinguishable from propositions on curvilinear motion and deflecting forces. Indeed, since all that we know of a deflecting force is the deflection which we ascribe to it, the employment of the word *force* in such discussions is little more than an abbreviation of language

This proposition being, by its services in explaining the phenomena of nature, the most valuable mechanical theorem ever given to the world, we may believe that much attention has been given to it, and that many methods of demonstrating it have been offered to the choice of mathematicians, the authors claiming some merit in facilitating or improving the investigation. Newton's demonstration is very short, but is a good deal encumbered with composition of ratios, and an arithmetical or algebraical turn of expression, frequently mixed with ideas purely geometrical. Newton was obliged to compress into it some properties of the conic sections which were not very familiar at that time, because not of frequent use: they are now familiar to every student, making part of the treatises of conic sections. By referring to these, the succeeding authors gave their demonstrations the appearance of greater simplicity and elegance. But Newton gives another demonstration in the second and third editions of the *Principia*, employing the deflective chord of the equicurve circle precisely in the way employed in our text. This mode of demonstration has been varied a little, by employing the radius of curvature, instead of the chord passing through the centre of forces. The theorems given by M. De Moivre were the first in this way, and are very general, and very elegant. Those of Jo. Bernoulli, Hermann, and Keill, scarcely differ from them, and none of them all is preferable to Newton's, now mentioned, either for generality, simplicity, or elegance.

205. It remains now to inquire, whether there be any analogy between the forces which retain the different planets in their respective orbits. It is highly probable that there is, seeing they all respect the Sun.' But it is by no means certain. Different bodies exhibit very different laws of action. Those of magnetism, electricity, and cohesion, are extremely different; and the chymical affinities, considered as the effects of attractive and repulsive forces, are as various as the substances themselves. As we know nothing of the constitution of the heavenly bodies, we cannot, *a priori*, say that it is not so here. Perhaps the planets are deflected by the impulsion of a fluid in motion, or are thrust toward the Sun by an elastic æther, denser and more elastic as we recede from the Sun. The Sun may be a magnet, and at the same time electrical. The Sun so constituted would act on a magnetical planet both by magnetical and

electrical attraction, while another planet is affected only by his electricity. A thousand such suppositions may be formed, all very possible. Newton therefore could not leave this question undecided.

Various means of deciding it are offered to us by the phenomena. The motion of the comets, and particularly of the Halleyan comet, seems to decide it at once. This comet came from a distance, far beyond the remotest of the known planets, and came nearer to the Sun than Venus. Therefore we are entitled to say, that a force inversely as the square of the distance from the Sun, extends without interruption through the whole planetary spaces. But farther, if we calculate the deflection actually observed in the Halleyan comet, when it was at the same distance from the Sun as any of the planets, we shall find it to be precisely the same with the deflection of that planet. There can remain no doubt therefore, that it is one and the same force which deflects both the comet and the planet.

But Newton could not employ this argument. The motions of the comets were altogether unknown, and probably would have remained so, had he not discovered the sameness of the planetary force through its whole scene of influence. The fact is, that Newton's first conjectures about the law of the solar force were founded on much easier observations.

Kepler's third law is, that *the squares of the periodic times of the planets are in the same proportion with the cubes of their mean distances from the Sun.* Thus, Mars is nearly four times as far from the Sun as Mercury, and his period is nearly eight times that of Mercury. Now, $4^3=64, =8$.

The planets describe figures which differ very little from circles, whose radii are those mean distances. If they described circles, it would have been very easy to ascertain the proportion of the centripetal forces. For, by Dynamics we had $f \doteqdot \frac{d}{t^2}$. Now, in the planetary motions, we have

$t^2 \doteqdot d^3$. Therefore, in this case, $f \doteqdot \frac{d}{d^3}$, or $\doteqdot \frac{1}{d^2}$, that is, the forces which regulate the motions of the planets at their mean distances are inversely as the squares of those distances.

It was this notion (by no means precise) of the planetary force, which had first occupied the thoughts of young Newton, while yet a student at college—and, on no better authority than this, had he supposed that a similar analogy would be observed between the deflection of the Moon and that of a cannon ball. His disappointment, occasioned by his erroneous estimation of the bulk of this Earth, and his horror at the thoughts of any such controversies as his optical discoveries had engaged him in, seem to have made him resolve to keep these thoughts to himself. But when Picard's measure of the Earth had removed his cause of mistake, and he saw that the analogy did really hold with respect to the force reaching from the Earth to the Moon; he then thought it worth his while to study the subject seriously, and to investigate the deflection in the arch of an ellipse. His study terminated in the proposition demonstrated above—doubtless to his great delight. He was no longer contented with the vague guess which he had made as to the proportion of the forces which deflected the different planets. The orbit of Mars, and still more the orbit of Mercury, is too eccentric to be considered as a circle. Besides, at the mean distances, the radius vector is not perpendicular to the curve, as it is in a circle. He was now in a condition to compare the simultaneous deflections of any two planets, in any part of their orbits. This he has done. In the fifteenth proposition of the first book of the *Principia*, he demonstrates, that if the forces actuating the different planets are in the inverse duplicate ratio of the distances from the Sun, then the squares of the periodic times must be as the cubes of the mean distances. This being a matter of observation, it follows, conversely, that the forces are in this inverse duplicate ratio of the distances.

Thus was his darling object attained. But as this fifteenth proposition has some intricacy, it is not so clear as we should wish in an elementary course like ours. The same truth may be easily made appear in the following manner.

206. *If a planet, when at its mean distance from the Sun, be projected in a direction perpendicular to the radius vector, with the same velocity which it has in that point of its orbit, it will describe a circle round the Sun in the same time that it describes the ellipse.*

Let ABPD (Fig. 18.) be the elliptical orbit, having the Sun in the focus S. Let AP, BD, be the two axes, C the centre, A the aphelion, P the perihelion, and B, D, the two situations of mean distance. About S describe the circle BDM. Let BK and BN be very small equal arches of the circle and the ellipse, and let BE be one half of BS. BM, the double of B S, is the deflective chord of the circle of curvature in the point B of the orbit, and BE is one-fourth of that chord. Therefore the velocity in B is that which the force in B would generate by uniformly impelling the planet along BE. But a body projected with this velocity, in the direction BK, will describe the circle BKMD.

The arches BK and BN being equal, and described with equal velocities, will be described in equal times. The triangles BKS, BNS, having equal bases BK and BN, are proportional to their altitudes BS and BC (for the elementary arch BN may be considered as coinciding with the tangent in B, and BC is perpendicular to this tangent). But, because BS is equal to CA, the area of the circle BMD is to that of the ellipse ABPD as AC to BC, that is, as BS to BC, that is, as the triangle BKS to the triangle BNS. These triangles are therefore similar portions of the whole areas, and therefore, since they are described in equal times, the circle BMD and the ellipse ABPD will also be described in equal times.

Thus it appears that Newton's first conjecture was perfectly just. For if the planets, instead of describing their elliptical orbits, were describing circles at the same distances, and in the same times, they would do it by the influence of the same forces. Therefore since, in this case, we should have $t^2 \doteqdot d^3$, the forces will be proportional to d^2 inversely.

207. We now see that the forces which retain the different planets in their orbits are not different forces, but that all are under the influence of one force, which extends from the Sun in every direction, and decreases in intensity as the square of the distance from the Sun increases. The intensity at any particular distance is the same, in whatever direction the distance is taken. Although the planetary courses do not depart far from our ecliptic, the influence of the regulating force is by no means confined to that neighbourhood. Comets have been seen, which rise almost perpendicular to the ecliptic; and their orbits or trajectories occupy all quarters of the heavens.

This relation, in which they all stand to the Sun, may justly be called a cosmical relation, depending on their mutual constitution, which appears to be the same in them all. As this force respects the Sun, it may be called a SOLAR FORCE, in the same sense as we use the term magnetical force. All persons unaffected by peculiar philosophical notions, conceive magnetism distinctly enough by calling it *Attraction*. For, whatever it is, its effects resemble those of attraction. If we conceive the magnetical phenomena as effects of a tendency toward the magnet, inherent in the iron, we may conceive the planetary deflections as produced in the same way; but this also indicates a sameness in the constitution of all the planets. Or we may ascribe the deflections to the impulsions or pressure of an æther;

but this also indicates a sameness of constitution over the whole system.

Thus, whatever notion we entertain of what we have called a solar or a planetary force (and the observed law of action limits us to no exclusive manner of conceiving it), we see a power of nature, whether extrinsic, like the action of a fluid, or intrinsic, like tendencies or attractions, which fit the Sun and planets for a particular purpose, giving them a cosmical relation, and laws of action.

208. It is still more interesting to remark, that the satellites observe the same law of action. For, in the little systems of a planet and its satellites, we observe the same analogy between the distances and periodic times. In short, a centripetal force in the inverse duplicate ratio of the distance seems to be the bond by which all is held together.

209. As the analogy observed by Kepler between the distances of the revolving bodies and the periods of their revolutions, led Newton to the discovery of the law of planetary deflection; so, this law being established, we are led to the second and third fact observed by Kepler as its necessary consequences. It appears, that the periodic time of a planet under the influence of a force inversely as the square of the distance, depends on its mean distance alone, and will be the same, whether the planet describe a circle or an ellipse having any degree whatever of eccentricity. This, as was already observed, is the fifteenth proposition of the first book of Newton's Principia. Suppose the shorter axis B D of the ellipse A B P D (Fig. 19.) to diminish continually, the longer axis A P remaining the same. As the extremity B of the invariable line B S moves from B toward C, the extremity S will move toward P, and when B coincides with C, S will coincide with P, and the ellipse is changed into a straight line P A, whose length is twice the mean distance S B.

In all the successive ellipses produced by this gradual diminution of C B, the periodic time remains unchanged. Just before the perfect coincidence of B with C, the ellipse may be conceived as undistinguishable from the line P A. The revolution in this ellipse is undistinguishable from the ascent of the body from the perihelion P to the aphelion A, and the subsequent descent from A to P. Therefore a body under the influence of the central force will descend from A to P in half the time of the revolution in the ellipse ADPBA. Therefore the time of descending from any distance B S is half the period of a body revolving at half that distance from the Sun. By such means we can tell the time in which any planet would fall to the Sun. Multiply the half of the time of a revolution by the square root of the cube of $\frac{1}{2}$, that is, by the square root of $\frac{1}{8}$; the product is the time of descent. Or divide the time of half a revolution by the square root of the cube of 2, that is, by the square root of 8, that is, by 2,82847; or, which is the shortest process, multiply the time of a revolution by the decimal 0,176776;

	d.	h.
Mercury will fall to the Sun in	15	13
Venus	39	17
The Earth	64	10
Mars	121	0
Jupiter	290	0
Saturn	798	0
Georgian planet	5406	0
The Moon to this Earth	4	21

Cor. The squares of the times of falling to the Sun are as the cubes of the distances from him.

210. So far did Newton proceed in his reasonings from the observations of Kepler. But there remained many important questions to be decided, in which those observations offered no direct help.

It appeared improbable that the solar force should not affect the secondary planets. It has been demonstrated

(14.) that if a body P revolve round another body S, describing areas proportional to the times, while S revolves round some other body, or is affected by some external force, P is not only acted on by a central force directed to S, but is also affected by every accelerating force which acts on S.

While, therefore, the Moon describes areas proportional to the times round the Earth, it is not only deflected toward the Earth, but it is also deflected as much as the Earth is toward the Sun. For the Moon accompanies the Earth in all its motions. The same thing must be affirmed concerning the satellites attending the other planets.

And thus has Newton established a fourth proposition, namely,

The force by which a secondary planet is made to accompany the primary in its orbit round the Sun is continually directed to the Sun, and is inversely as the square of the distance from him. For, as the primary changes its distance from the Sun, the force by which it is retained in its orbit varies in this inverse duplicate ratio of the distance. Therefore the force which causes the secondary planet to accompany its primary *must* vary in the same proportion, in order to produce the same change in its motion that is produced in that of the primary. And, further, since the force which retains Jupiter in his orbit is to that which retains the Earth as the square of the Earth's distance is to that of Jupiter's distance, the forces by which their respective satellites are made to accompany them must vary in the same proportion.

Thus, all the bodies of the solar system are continually urged by a force directed to the Sun, and decreasing as the square of the distance from him increases.

211. Newton remarked, that in all the changes of motion observable in our sublunary world, the changes in the acting bodies are equal and opposite. In all impulsions, one body is observed to lose as much motion as the other

gains. All magnetical and electrical attractions and repulsions are mutual. Every action seems to be accompanied by an equal reaction in the opposite direction. He even imagined that it may be proved, from abstract principles, that it must be so. He therefore affirmed, that this law obtained also in the celestial motions, and that not only were the planets continually impelled toward the Sun, but also that the Sun was impelled toward the planets. The doubts which may be entertained concerning the authority of this law of motion have been noticed already. At present, we are to notice the facts which the celestial motions furnish in support of Sir Isaac Newton's assertion.

212. Directions have been given (56.) how to calculate the Sun's place for any given moment. When the astronomers had obtained instruments of nice construction, and had improved the art of observing, there was found an irregularity in this calculation, which had an evident relation to the Moon. At new Moon, the observations corresponded exactly with the Sun's calculated place; but seven or eight days after, the Sun is observed to be about 8" or 10" to the eastward of his calculated place, when the Moon is in her first quadrature, and he is observed as much to the westward when she is in the last quadrature. In intermediate situations, the error is observed to increase in the proportion of the sine of the Moon's distance from conjunction or opposition.

Things must be so, if it be true that the deflection of the Moon toward the Earth is accompanied with an equal deflection of the Earth toward the Moon. For the Moon will not revolve round the Earth, but the Earth and Moon will revolve round their common centre of position. When the Moon is in her first quadrature, her position may be represented by M (Fig. 20.) while the Earth is at E, and their common centre is at A. A spectator in A will see the Sun S in his calculated place B. But the spectator in the Earth E sees the Sun in C, to the left hand, or eastward of B. The interval BC measures the angle BSC, or ASE, subtended at the Sun by the distance EA of the common centre of the Earth and Moon from the centre of the Earth. At new Moon, A, E, and S, are in a straight line, so that B and C coincide. At the last quadrature, the Moon is at *m*, the Earth at *e*, and the common centre at *a*. Now the Sun is seen at *c*, 8″ or 10″ to the westward of his calculated place. This correction has been pointed out by Newton, but it was not observed at the first, owing to its being blended with the Sun's horizontal parallax, which had not been taken into account. But it was soon recognised, and it now makes an article among the various equations used in calculating the Sun's place.

Here, then, is a plain proof of a mutual action and reaction of the Earth and Moon. For, since they revolve round a common centre, the Earth is unquestionably deflected into the curve line by the action of a force directed towards the Moon. But we have a much better proof. The waters of the ocean are observed every day to heap up on that part of our globe which is under the Moon. In this situation, the weight of the water is diminished by the attraction of the Moon, and it requires a greater elevation, or a greater quantity, to compensate for the diminished weight. On the other hand, we see the waters abstracted from all those parts which have the Moon in the horizon. Kepler, after asserting, in very positive terms, that the Earth and Moon would run together, and are prevented by a mutual circulation round their common centre, adduces the tides as a proof.

213. As the art of observation continued to improve, astronomers were able to remark abundant proofs of the tendency of the Sun toward the planets. When the great planets Jupiter and Saturn are in quadrature with the Earth, to the right hand of the line drawn from the Earth to the Sun's calculated place, the Sun is then observed to shift to the left of that line, keeping always on the opposite

side of the common centre of position. These deviations are indeed very minute, because the Sun is vastly more massive than all the planets collected into one lump. But in favourable situations of these planets, they are perfectly sensible, and have been calculated; and they *must* be taken into account in every calculation of the Sun's place in order to have it with the accuracy that is now attainable. It must be granted that this accuracy, actually attained by means of those corrections, and unattainable without them, is a positive proof of this mutual deflection of the Sun toward the planets. The quantity corresponding to one planet is too small, of itself, to be very distinctly observed; but, by occasionally combining with others of the same kind, the sum becomes very sensible, and susceptible of measure. It sometimes amounts to 38 seconds, and must never be omitted in the calculations subservient to the finding the longitude of a ship at sea. Philosophy, in this instance, is greatly indebted to the arts. And she has liberally repaid the service.

214. Here it is worthy of remark, that had the Sun been much smaller than he is, so that he would have moved much further from the common centre, and would have been much more agitated by the tendencies to the different planets, it is probable that we never should have acquired any distinct or useful knowledge of the system. For we now see that Kepler's laws cannot be strictly true; yet it was those laws alone that suggested the thought, and furnished to young Newton the means of investigation. The analogy of the periodic times and distances is accurate, only with respect to the common centre, but not with respect to the Sun. But the great mass of the Sun occasions this common centre to be generally within his surface, and it is never distant from it one-fourth of his diameter. Therefore this third law of Kepler is so nearly exact in respect of the Sun, that the art of observation, in Newton's lifetime, could not have found any errors. The penetrating eye of New-

ton however immediately perceived his own good fortune, and his error in supposing Kepler's laws accurately true. But this was not enough for his philosophy; he was determined that it should narrate nothing but truth. With great ingenuity, and elegance of method, he demonstrates that his mechanical inferences from Kepler's laws are still strictly true, and that his own law of planetary force is exact, although the centre of revolution is not the centre of the Sun. All the difference respects the absolute magnitude of the periodic times in relation to the magnitude of the force. This he demonstrates in a series of propositions, of which we have given the chief.

215. Newton proceeds still further in his investigation of the extent of the influence of this planetary force, and says that *all the planets mutually tend toward each other.* It does not appear how this opinion arose in his mind. There are abundance of phenomena, however, of easy observation, which make it very evident. It was probably a conjecture, suggested by observing this reciprocal action between the Earth and Moon. But he immediately followed it into its consequences, and pointed them out to the astronomers. They are very important, and explain many phenomena which had hitherto greatly perplexed the astronomers.

Suppose Jupiter and Mars to be in conjunction, lying in the same line from the Sun. As Mars revolves much quicker than Jupiter, he gets before him, but, being attracted by Jupiter, his motion is retarded—and Jupiter, being attracted by Mars, is accelerated. On the contrary, before Mars arrives at conjunction with Jupiter, Mars is accelerated, and Jupiter is retarded. Further, the attraction of Mars by Jupiter must diminish the tendency of Mars to the Sun, or must act in opposition to the attraction of the Sun; therefore the curvature of Mars's orbit in that place must be diminished. On the contrary, the tendency of Jupiter to Mars, acting in the same direction as his tendency to the Sun, must increase the curvature of that part of Jupiter's orbit. If Jupiter be at this time advancing to his aphelion, this increase of curvature will sooner bend the line of his motion from an obtuse into a right angle with the radius vector. Therefore his aphelion will be sooner attained, and it will appear to have shifted to the westward. For the opposite reasons, the apsides of Mars will seem to shift to the eastward. There are other situations of these planets where the contrary effects will happen. In each revolution, each planet will be alternately accelerated twice, and twice retarded, and the apsides of the exterior planet will continually recede, and that of the interior will advance. It is obvious that this disturbance of the motion of a planet by its deflection to another, though probably very minute, yet being continued for a tract of time, its accumulated result may become very sensible. These changes are all susceptible of accurate calculation, as we shall afterwards explain particularly.

This must be considered as a convincing proof of the mutual action of the heavenly bodies, and it adds fresh lustre to the penetration and genius of Newton, who made these assertions independent of observation, pointing out to astronomers the sure means of perfecting their knowledge of the celestial motions.

216. Here therefore we have established a fifth proposition in physical astronomy, namely, that *that all the bodies in the solar system tend mutually toward one another, with forces which vary in the inverse duplicate ratio of the distances.*

It did not satisfy Newton that he merely pointed out the gross effect of this mutual tendency. He gave astronomers the means of investigating and ascertaining its intensity, and its variation by a variation of distance. The effect of the Earth's tendency to Jupiter during any length of time, may be computed by means of Newton's dynamical propositions, contained in the first book of his Principia, particularly by the 39th. Of these we have given a proper selection in the general doctrines of Dynamics.

217. But the inquisitive mind of Newton did not stop here. He was anxious to learn whether this planetary tendency had any resemblance or relation to forces with which we are more familiarly acquainted. Of this kind are magnetism and gravity. He was the more incited to this investigation by the conjectures on this subject which had arisen in the mind of Kepler. This great astronomer had been much taken with the discovery just published by Dr Gilbert of Colchester, stating that this Earth is a great magnet, and he was disposed to ascribe the revolution of the Moon to the magnetical influence of the Earth. It appears from Newton's papers, that he had made a great many experiments for discovering the law of magnetic action. But he had found it so dependant on circumstances of form and situation, and so changeable by time, that it seemed susceptible of no comparison with the solar force; and he soon gave it up. He was more successful in tracing the resemblances observable in the phenomena of common gravity. It has been already remarked (197.) that, very early in life, he had conjectured that it was the same with the solar force, and that after he had formed the opinion that the solar force varied in the inverse duplicate ratio of the distance, he put his conjecture to the test, by comparing the fall of a stone with the deflection of the Moon. The distance of the Moon is estimated to be 60 semidiameters of the Earth. Therefore, if gravity and the lunar deflecting force be the same, the stone should deflect as much in one second as the Moon does in a minute. For we may, without any sensible error, suppose that the lunar force acts uniformly during one minute. If so, the linear deflections must be as the squares of the times. The deflection in a minute must be 60 × 60 times, or 3600 times the deflection in a second. But, according to the law of planetary force, the deflection at the Earth's surface must be 60 × 60,

or 3600 times the deflection at the Moon. Now, in a second, a stone falls 16 feet and an inch. Therefore the Moon should deflect 16 feet and an inch in a minute from the tangent of her orbit. Newton calculated the versed sine of the arch described by the Moon in a minute, to a radius equal to 60 semidiameters of the Earth. He found it only about 13$\frac{1}{2}$ feet, and he gave over any farther inquiry. But he had hastily supposed a degree to contain 60 miles, not attending to the difference between a geographical mile or 60th of a degree, and an English statute mile. A degree contains 69 such miles; so that he had made the Moon's orbit, and consequently her deflection, too small in the same proportion. If we increase the calculated deflection in this proportion, it comes out exactly 16$\frac{1}{12}$; and the conjecture is fully established.

When Picard's accurate measure of the Earth had enabled Newton to confirm his former conjecture concerning the identity of the planetary force and terrestrial gravity, he again made the calculation and comparison *in the most scrupulous manner*. For we now see that several circumstances must be taken into the account, which he had omitted in his first computation from Picard's measure of the Earth. The fall in a second is not the exact measure of terrestrial gravity. A stone would fall farther, were it not that its gravity is diminished by the Earth's rotation. It is also diminished by the action of the Sun and Moon, and by the weight of the air which the stone displaces. All these diminutions of the accelerating force of gravity are susceptible of exact calculation, and were accordingly calculated by Newton, and the amount added to the observed acceleration of a falling body. In the next place, the real radius of the Moon's orbit must be reckoned only from the common centre of the Earth and Moon. And then the force deduced from this deflection must be increased in the subduplicate ratio of the matter in the Earth to the matter in the Earth and Moon added together

All this has been done, and the result coincides precisely with observation.

This may be demonstrated in another way. We can tell in what time a body would revolve round the Earth, close to its surface. For we must have t^2 proportional to d^3. It will be found to be 84 minutes and 34 seconds. Then we know the arch described in one second, and can calculate its deflection from the tangent. We shall find it $16\frac{1}{12}$ feet, the same with that produced by common gravity.

218. *Terrestrial gravity, therefore, or that force which causes bodies to fall, or to press on their supports, is only a particular example of that universal tendency, by which all the bodies of the solar system are retained in their orbits.*

We must now extend to those bodies the other symptoms of common gravity. It is by gravity that water arranges itself into a level surface; that is, a surface which makes a part of the great sphere of the ocean. The weight of this water keeps it together in a round form. We must ascribe the globular forms of the Sun and planets to a similar operation. A body on their surface will press it as a heavy body presses the ground. Dr Hooke remarks, that all the protuberances on the surface of the Moon are of forms consistent with a gravity toward its centre. They are generally sloping, and, though in some places very rugged and precipitous, yet nowhere overhang, or have any shape that would not stand on the ground. The more rugged parts are most evidently matter which has been thrown up by volcanic explosion, and have fallen down again by their lunar gravity.

219. That property by which bodies are heavy is called GRAVITY, HEAVINESS—the being heavy; and the *fact* that it moves toward the Earth, may be called GRAVITATION. While it falls, or presses on its supports, it may be said to *gravitate*, to give indication of its being *gravis* or heavy. In this sense the planets *gravitate* to the Sun, and the se-

condary planets to their primaries; and, in short, every body in the solar system to every other body. By the verb *to gravitate*, nothing is meant but the fact, that they either actually approach, or manifest, by a very sensible pressure, tendencies to approach the body to which they are said to gravitate. The verb, or the noun, should not be considered as the expression of any quality or property, but merely of a phenomenon, a fact or event in nature.

220. But this deviation from uniform rectilineal motion is considered as an *effect*, and it is of importance to discover the *cause*. Now, in the most familiar instance, the fall or pressure of a heavy body, we ascribe the fall, or pressure indicating the tendency to fall, to its heaviness. But we have no other notion of this heaviness than the very thing which we ascribe to it as an effect. The feeling the heaviness of the piece of lead that lies in our hand, is *the sum of all that we know about it*. But we consider this heaviness as a *property* of all terrestrial matter, because all bodies give some of those appearances which we consider as indications of it. All move toward the Earth if not supported, and all press on the support. The feeling of pressure which a heavy body excites might be considered as its characteristic phenomenon; for it is this feeling that makes us think it a force—we must oppose our force to it; but we cannot distinguish it from the feeling of any other equal pressure. It is most distinguishable as the cause of motion, as a moving or accelerating force. In short, we know nothing of gravity but the phenomena, which we consider, not as gravity, but as its indication. It is, like every other force—an unknown quality.

The *weight* of a body should be distinguished from its gravity or heaviness, and the term should be reserved for expressing the measure of the united gravitation of all the matter in the body. This is indeed the proper sense of the term *weight*—*pondus*. In ordinary business, we measure the weights of bodies by means of known units of weight.

A piece of lead is said to be of twenty pounds weight when it balances twenty pieces of matter, each of which is a pound; but we frequently measure it by means of other pressures, as when we judge of it by the division to which it draws the scale of a spring steelyard.

221. We estimate the quantity of matter in a body by its weight, and say that there is nineteen times as much matter in a cubic foot of gold as there is in a cubic foot of water. This evidently presupposes that *all matter is heavy, and equally heavy*—that every primitive atom of matter is equally heavy. But this seems to be more than we are entitled to say, without some positive proof. There is nothing inconceivable or absurd in supposing one atom to be twice or thrice as heavy as another. As gravity is a contingent quality of matter, its absolute strength or force is also contingent and arbitrary. We can conceive an atom to have no weight. Nay, we can as clearly conceive an atom of matter to be endowed with a tendency upwards as with a tendency downwards. Accordingly, during the prevalence of the Stahlian doctrine of combustion, that matter which imparts inflammability to bodies was supposed to be not only without weight, but positively light, and to diminish the weight of the other ingredients with which it was combined in a combustible body. In this way, the abettors of that doctrine accounted for the increase of weight observable when a body is burnt.

There is nothing absurd or unreasonable in all this; and had we no other indication of gravity but its pressure, we do not see how this question can be decided. But gravity is not only a pressing power, but also a moving or accelerating power. If a body consisted of a thousand atoms of gravitating matter, and as many atoms of matter which does not gravitate, and if the gravity of each atom exerted the pressure of one grain, this body would weigh a thousand grains, either by a balance or a spring steelyard, yet it contains two thousand atoms of matter. But take another body of the same weight, but consisting wholly of gravitating atoms; drop these two bodies at once from the hand—the last mentioned will fall 16 feet in the first second—the other will fall only 8 feet. For in both there is the same moving force; therefore the same quantity of motion will be produced in both bodies; that is, the products of the quantities of matter by the velocities generated will be the same. Therefore the velocity acquired by the mixed body will be one half of that acquired in the same time by the simple body. The phenomenon will be what was asserted, one will fall 16 and the other only 8 feet.

This will be still more forcibly conceived, if we take two bodies a and b, each containing 1000 atoms of gravitating matter, and attach a to another body c, containing 1000 atoms which do not gravitate. Now, unless we suppose c moveable and arrestable by a thought or a word, we can have no hesitation in saying, that the mass $a + c$ will fall with half the velocity of b.

We see therefore that the *accelerating power* alone of gravity enables us to decide the question, 'whether all terrestrial matter gravitates,' and gravitates alike. We have only to try whether all terrestrial bodies fall equally far in the same time, or receive an equal increment of velocity in the same time. This test of the matter did not escape the penetrating genius of young Newton. He made experiments on every kind of substance, metals, stones, woods, grain, salts, animal substances, &c. and made them in a way susceptible of the utmost accuracy, as we shall see afterwards. The result was, that all these substances were equally accelerated; and, on this authority, Newton thought himself entitled to say that ALL TERRESTRIAL MATTER IS EQUALLY HEAVY.

This however may be disputed. For it is plain, that if all bodies contain *an equal proportion* of gravitating and nongravitating matter, they will be equally accelerated; nay, the unequal gravitation of different substances, and even positive levity, may be so compensated by the proportion of those different kinds of matter, that the total gravitation may still be proportional to the whole quantity of matter.

But, till we have some authority for saying that there is a difference in the gravitation of different atoms, the just rules of philosophical discussion oblige us to believe that all gravitate alike. This is corroborated by the universality of the law of mutual and equal reaction. This is next to demonstration, that the primitive atoms are alike in every respect, and therefore in their gravitation.

We are entitled therefore to say, that all terrestrial matter is equally heavy, and that the weight of a body is the measure of the united gravitation of every atom, and therefore is a measure of, or is proportional to, the quantity of matter contained in it.

222. Newton naturally, and justly, extended the affirmation to the planets and to the Sun. But here arises a question, at once nice and important. The law of gravitation, so often mentioned, is exhibited in the mutual deflections of great masses of matter. These deflections are in the inverse duplicate ratio of the distances between the centres of the masses. Are we warranted by this observation to say, that this is also the law of action between every atom of one body and every atom of another? Can we say in general that the law of corpuscular action is the same with that of masses, resulting from the combined action of each atom on each? We are assured by experience that it is not. For we observe that, in magnets, the law of action (that is, the relation subsisting between the distances and the intensities of force) is different in almost every different magnet, and seems to depend in a great measure on their form.

Newton was too cautious, and too good a logician, to advance such a proposition without proof; and therefore, confining himself to the single case of sperical and spheroidal bodies, the forms in which we observe the planetary masses to be compacted, he inquired what sensible action between the masses will result from an action between their particles inversely proportional to the square of their distances.

Let ALBM, $albm$ (Fig. 21.) be two spherical surfaces, of which C is the common centre, and let the space between them be filled with gravitating matter, uniformly dense. Let p be a particle placed any where within this spherical shell, to every particle of which it gravitates with a force inversely as the square of its distance from it. This particle will have no tendency to move in any direction, because its gravitation in any one direction is exactly balanced by an equal gravitation in the opposite direction.

Draw through p the two straight lines $dp\epsilon$, $ep\delta$, making a very small angle at p. This may represent the section of a very slender double cone dpe, $\delta p\epsilon$, having p for the common vertex, and de, $\delta\epsilon$ for the diameters of the circular bases. The gravitation of p to the matter in the base de is equal to its gravitation to the matter in the base $\delta\epsilon$. For the number of particles in de is to the number in $\delta\epsilon$ as the surface of the base de to that of the base $\delta\epsilon$, that is, as de^2 to $\delta\epsilon^2$, that is, as pd^2 to $p\delta^2$, that is, as the gravitation to a particle in $\delta\epsilon$ to the gravitation to a particle in de. Therefore the whole gravitation to the matter in de is the same with the whole gravitation to the matter in $\delta\epsilon$—since it is also in the opposite direction, the particle p is in equilibrio. The same thing may be demonstrated of the gravitation to the matter in qr and st, and, in like manner, of the gravitation to the matter in the sections of the cones dpe, $\delta p\epsilon$ by any other concentric surface. Consequently, the gravitation to the whole matter contained in the solid $dqre$ is equal to the gravitation to the whole matter in the solid $\delta ts\epsilon$, and the particle p is still in equilibrio.

Now, since the lines $dp\epsilon$, $ep\delta$ may be drawn in any direction, and thus be made to occupy the whole sphere, it is evident that the gravitation of p is balanced in every direction, and therefore it has no tendency to move in any direction in consequence of this gravitation to the spherical shell of matter comprehended between the surfaces A L B M and $a\,l\,b\,m$.

It is also evident, that this holds true with respect to all the matter comprehended between A L B M and the concentric surface $p\,v\,n$ passing through p; in short, p is in equilibrio in its gravitation to all the matter more remote than itself from the centre of the sphere, and appears as if it did not gravitate at all to any matter more remote from the centre.

223. We have supposed the spherical shell to be uniformly dense. But p will still be in equilibrio, although the shell be made up of concentric strata of different density, provided that each stratum be uniformly dense. For should we suppose, that, in the space comprehended between A L B M and $p\,n\,v$, there occurs a surface $a\,l\,b\,m$ of a different density from all the rest, the gravitation to the intercepted portions $q\,r$ and $s\,t$ are equal, because these portions are of equal density, and are proportional to $p\,q^2$ and $p\,s^2$ inversely. The proposition may therefore be expressed in the following very general terms: ‘ *A particle* ‘ *placed any where within a spherical shell of gravitating* ‘ *matter, of equal density at all equal distances from the* ‘ *centre, will be in equilibrio, and will have no tendency* ‘ *to move in any direction.*’

Remark.—The equality of the gravitation to the surface $e\,d$ and to the surface ɩ ȝ is affirmed, because the numbers of particles in the two surfaces are inversely as the gravitations towards one in each. For the very same reason, the gravitations to the surfaces $e\,d$, and $q\,r$, and $t\,s$, are all equal. Hence may be derived an elementary proposition, which is of great use in all inquiries of this kind, namely,

224. If a cone or pyramid $d\,p\,e$, of uniform gravitating matter, be divided by parallel sections, $d\,e$, $q\,r$, &c. the

gravitation of a particle p in the vertex to each of those sections is the same, and the gravitations to the solids $p q r$, $p d e$, $q d e r$, &c. are proportional to their lengths $p q$, $p d$, $q d$, &c. The first part of this proposition is already demonstrated. Now, conceive the cone to be thus divided into innumerable slices of equal thickness. It is plain, that the gravitation to each of these is the same, and therefore the gravitation to the solid $q p r$ is to the gravitation to the solid $q d e r$ as the number of slices in the first to the number in the second, that is, as $p q$, the length of the first, to $q d$, the length of the second.

The cone $d p e$ was supposed extremely slender. This was not necessary for the demonstration of the particular case, where all the sections were parallel. But in this elementary proposition, the angle at p is supposed smaller than any assigned angle, that the cone or pyramid may be considered as one of the elements into which we may resolve a body of any form. In this resolution, the bases are supposed, if not otherwise expressly stated, to be parallel, and perpendicular to the axes; indeed they are supposed to be portions $x r$, $y e$, $z \varepsilon$, &c. of spherical surfaces, having their centres in p. The small portions $x r q$, $y e d$, $z \varepsilon \delta$, &c. are held as insignificant, vanishing in the ultimate ratios of the whole solids.

It is easy, also, to see, that the equilibrium of p is not limited to the case of a spherical shell, but will hold true of any body composed of parallel strata, or strata so formed that the lines $p d$, $p \delta$ are cut in the same proportion by the sections $d e$, $q r$, &c. In a spheroidal shell, for example, whose inner and outer surfaces are similar, and similarly posited spheroids, the particle p will be in equilibrio any where within it, because, in this case, the lines $p \gamma$ and $n e$ are equal; so are the lines $p \varepsilon$ and $o d$, the lines $t \gamma$ and $r e$, the lines $s \varepsilon$ and $q d$, &c. In most cases, however, there is but one situation of the particle p that will ensure this equilibrium. But we may, at the same time, infer the following very useful proposition.

225. *If there be two solids perfectly similar, and of the same uniform density, the gravitation to each of these solids, by a particle similarly placed on or in each, is proportional to any homologous lines of the solids.*

For, the solids being similar, they may be resolved into the same number of similar pyramids similarly placed in the solids. The gravitations to each of any corresponding pair of pyramids are proportional to the lengths of those pyramids. These lengths have the same proportion in every corresponding pair. Therefore the absolute gravitations to the whole pyramids of one solid has the same ratio to the absolute gravitation to the whole pyramids of the other solid. And, since the solids are similar, and the particles are at the similarly placed vertexes of all the similar and similarly placed pyramids, the gravitation compounded of the absolute gravitations to the pyramids of one solid has the same ratio to the gravitation similarly compounded of the absolute gravitations to the pyramids of the other.

226. *The gravitation of an external particle to a spherical surface, shell, or entire sphere, which is equally dense at all equal distances from the centre, is the same as if the whole matter were collected in its centre.*

Let A L B M (Fig. 21.) represent such a sphere, and let P be the external particle. Draw P A C B through the centre C of the sphere, and cross it by L C M at right angles. Draw two right lines P D, P E, containing a very small angle at P, and cutting the great circle A L B M in D, E, D′, E′. About P as a centre, with the distance P C, describe the arch C *d m*, cutting D P in *d*, and E P in *e*. About the same centre describe the arc D O. Draw *d* F, *e* G parallel to A B, and cutting L C in *f* and *g*. Draw C K perpendicular to P D, and *d* H, D *δ*, and F I *φ* perpendicular to A B. Join C D and C F.

Now, let the figure be supposed to turn round the axis P B. The semicircumference A L B will generate a complete spherical surface. The arch C *d m* will generate another spherical surface, having P for its centre. The small arches D E, *d e*, F G will generate rings or zones of those spherical surfaces. D O will also generate a zone of a surface, having P for its centre: *f g* and F I will generate zones of flat circular surfaces.

It is evident, that the zones generated by D E and D O (which we may call the zones D E and D O), having the same radius D δ, are to each other as their respective breadths D E and D O. In like manner, the zones generated by *d e*, *f g*, F I, F G, being all at the same distance from the axis A B, are also as their respective breadths *d e*, *f g*, F I, F G. But the zone D O is to the zone *d e* as PD^2 to Pd^2. For D O is to *d e* as P D to P *d*, and the radius of rotation D δ is to the radius *d* H, also as P D to P *d*. The circumferences described by D O and *d e* are therefore in the same proportion of P D to P *d*. Therefore the zones, being as their breadths and as their circumferences jointly, are as PD^2 and Pd^2.

C K and *d* H, being the sines of the same arch C *d*, are equal. Therefore K D and *f* F, the halves of chords equally distant from the centre, are also equal. Therefore the triangles C D K and C F *f* are equal and similar. But C D K is similar to E D O. For the right angles P D O and C D E are equal. Taking away the common angle C D O, the remainders C D K and E D O are equal. In like manner, C F *f* and G F I are similar, and therefore (since C D K and C F *f* are similar) the elementary triangles E D O and G F I are similar, and D O : D E = F I : F G.

The absolute gravitation or tendency of P to the zone D O is equal to its absolute gravitation to the zone *d e*, because the number of particles of the first is to the number in the last in PD^2 to Pd^2, that is, inversely, as the gravitation to a particle in the first to the gravitation to a particle in the last. Therefore let c express the circumference of a circle whose radius is 1. The surface of the zone generated by D O will be $\mathrm{DO} \times c \times \mathrm{D}\partial$, and the gravitation to it will be $\frac{\mathrm{DO} \times c \times \mathrm{D}\partial}{\mathrm{PD}^2}$, to which $\frac{de \times c \times d\mathrm{H}}{\mathrm{P}d^2}$, or $\frac{de \times c \times d\mathrm{H}}{\mathrm{PC}^2}$ is equal. This expresses the absolute gravitation to the zone generated by D O, this gravitation being exerted in the direction P D.

But it is evident that the tendency of P, arising from its gravitation to every particle in the zone, must be in the direction P C. The oblique gravitation must therefore be estimated in the direction P C, and must be reduced, in the proportion of P d to P H. It is plain that $\mathrm{P}d : \mathrm{PH} = de : fg$, because de and fg are perpendicular to P d and P H. Therefore the reduced or central gravitation of P to the zone generated by D O will be expressed by $\frac{fg \times c \times d\mathrm{H}}{\mathrm{PC}^2}$.

But the gravitation to the zone generated by D O is to the gravitation to the zone generated by D E, as D O to D E, that is, as F I (or fg) to F G. Therefore the central gravitation to the zone generated by D E will be expressed by $\frac{\mathrm{FG} \times c \times d\mathrm{H}}{\mathrm{PC}^2}$. Now $\mathrm{FG} \times c \times d\mathrm{H}$ is the value of the surface of the zone generated by F G. And if all this matter were collected in C, the gravitation of P to it would be exactly $\frac{\mathrm{FG} \times c \times d\mathrm{H}}{\mathrm{PC}^2}$, and it would be in the direction P C. Hence it follows, that the central gravitation of P to the zone generated by DE, is the same as its gravitation to all the matter in the zone generated by F G, if that matter were placed in C.

What has been demonstrated respecting the arch D E, is true of every portion of the circumference. Each has a substitute F G, which being placed in the centre C, the gravitation of P is the same. If P T touch the sphere in T, every portion of the arch T L B will have its substitute in the quadrant L B, and every part of the arch A T has its substitute in the quadrant A T L, as is easily seen. And hence it follows, that the gravitation of a particle P to a spherical surface A L B M is the same as if all the matter of that surface were collected in its centre.

We see, also, that the gravitation to the surface generated by the rotation of A T round A B is equal to the gravitation to the surface generated by T L B, which is much larger, but more remote.

What we have now demonstrated with respect to the surface generated by the semicircle A L B is equally true with regard to the surface generated by any concentric semicircle, such as *a l b*. It is true, therefore, in regard to the shell comprehended between those surfaces; for this shell may be resolved into innumerable concentric strata, and the proposition may be affirmed with respect to each of them, and therefore with respect to the whole. And this will still be true, if the whole sphere be thus occupied.

Lastly, it follows, that the proposition is still true, although those strata should differ in density, provided that each stratum is uniformly dense in every part.

It may therefore be affirmed, in the most general terms, that a particle, P, placed without a spherical surface, shell, or entire sphere, equally dense at equal distances from the centre, tends to the centre with the same force as if the whole matter of the surface, shell, or sphere, were collected there.

This will be found to be a very important proposition, greatly assisting us in the explanation of abstruse phenomena in other departments of natural philosophy.

227. *The gravitation of an external particle to a spherical surface, shell, or entire sphere, of uniform density, at equal distances from the centre, is as the quantity of mat-*

ter in that body, directly, and as the square of the distance from its centre, inversely.

For if all the matter were collected in its centre, the gravitation would be the same, and it would then vary in the inverse duplicate ratio of the distance.

228. *Cor.* 1. Particles placed on the surface of spheres of equal density, gravitate to the centres of those spheres with forces proportional to the radii of the spheres.

For the quantities of matter are as the cubes of the radii. Therefore the gravitation g is as $\frac{d^3}{d^2}$, that is, as d. This is a particular case of Prop. 225.

229. *Cor.* 2. The same thing holds true, if the distance of the external particles from the centres of the spheres are as the diameters or radii of the spheres.

230. *Cor.* 3. If a particle be placed within the surface of a sphere of uniform density, its gravitation, at different distances from the centre, will be as those distances. For it will not be affected by any matter of the sphere that is more remote from the centre (466.); and its gravitation to what is less remote is as its distance from the centre by the last corollary.

231. *The mutual gravitation of two spheres of uniform density in their concentric strata is in the inverse duplicate ratio of the distance between their centres.*

For the gravitation of each particle in the sphere A to to the sphere B, is the same as if all the matter in B were collected at its centre. Suppose it so placed. The gravitation of B to A will be the same as if all the matter in A were collected in its centre. Therefore it will be as d^2 inversely. But the gravitation of A to B is equal to that of B to A. Therefore, &c.

232. The absolute gravitation of two spheres, whose quantities of matter are a and b, and d the distance of their centres, is $\frac{a \times b}{d^2}$. For the tendency of one particle of a

to b, being the aggregate of its tendencies to every particle of b, is $\frac{b}{d^2}$. Therefore the tendency of the whole of a to b must be $\frac{a \times b}{d^2}$. And the tendency of b to a is equal to that of a to b.

233. This consequence of a mutual gravitation between particles proportional to $\frac{1}{d^2}$, is agreeable to what is observed in the solar system. The planets are very nearly spherical, and they are observed to gravitate mutually in this proportion of the distance between their centres. This mutual action of two spheres could not result from any other law of action between the particles. Therefore we conclude, that the particles of gravitating matter, of which the planets are formed, gravitate to each other according to this law, and that the observed gravitation of the planets is the united effect of the gravitation of each particle to each. There is just one other case, in which the law of corpuscular action is the same with the law of action between the masses; and this is when the mutual action of the corpuscles is as their distance directly. But no such law is observed in all the phenomena of nature.

The general inference, drawn by Sir Isaac Newton from the phenomena, may be thus expressed: *Every particle of matter gravitates to every other particle of matter with a force inversely proportional to the square of the distance from it.* Hence this doctrine has been called THE DOCTRINE OF UNIVERSAL GRAVITATION.

The description of a conic section round the focus, fully proves that this law of the distances is the law competent to all the gravitating particles. But whether all particles gravitate, and gravitate alike, is not demonstrated. The analogy between the distance of the different planets and their periodic times only proves, that the total gravitation of the different planets is in the same proportion with their quantity of matter. For the force observed by us, and found to be in the inverse duplicate ratio of the distance of the planet, is the *accelerating* force of gravity, being measured by the acceleration which it produces in the different planets. But if one half of a planet be matter which does not gravitate, and the other half gravitates twice as much as the matter of another planet, these two planets will still have their periods and distances agreeable to Kepler's third law. But since no phenomenon indicates any inequality in the gravitation of different substances, it is proper to admit its perfect equality, and to conclude with Sir Isaac Newton.

234. The general consequence of this doctrine is, that any two bodies, at perfect liberty to move, should approach each other. This may be made the subject of experiment, in order to see whether the mutual tendencies of the planets arise from that of their particles. For it must still be remembered, that although this constitution of the particles will produce this appearance, it may arise from some other cause.

Such experiments have accordingly been made. Bodies have been suspended very nicely, and they have been observed to approach each other. But a more careful examination of all circumstances has shewn, that most of those mutual approaches have arisen from other causes. Several philosophers of reputation have therefore refused to admit a mutual gravitation as a phenomenon competent to all matter.

But no such approach should be observed in the experiments now alluded to. The mutual approach of two spheres A and B, at the distance D of their centres, must be to the approach to the Earth E at the distance d of their centres in the proportion of $\frac{A \times B}{D^2}$ to $\frac{A \times E}{d^2}$, that is, of $\frac{B}{D^2}$ to $\frac{E}{d^2}$. Therefore, if a particle be placed at the surface of a golden sphere one foot in diameter, its gravitation to the Earth must be more than ten millions of times greater than its gravitation to the gold. For the diameter of the Earth is nearly forty millions of feet, and the density of gold is nearly four times the mean density of the Earth. And therefore, in a second, it would approach less than the ten millionth part of 16 feet—a quantity altogether insensible.

If we could employ in these experiments bodies of sufficient magnitude, a sensible effect might be expected. Suppose T (Fig. 22.) to be a ball of equal density with the Earth, and two geographical miles in diameter, and let the particle B be at its surface. Its gravity to T will be to its gravitation to the Earth nearly as 1 to 2300; and, therefore, if suspended like a plummet, it would certainly deviate 1′ from the perpendicular. A mountain two miles high, and hemispherical, rising in a level country, would produce the same deviation of the plummet.

235. Accordingly, such deviation of a plumb line has been observed. First, by the French academicians employed to measure a degree of the meridian in Peru. Having placed their observatories on the north and south sides of the vast mountain Chimboraço, they found, that the plummets of their quadrants were deflected toward the mountain. Of this they could accurately judge, by means of the stars which they saw through the telescope of their quadrant, when they were pointed vertically by means of the plummet.

Thus, if the plummets take the positions A B, C D, (Fig. 23.) instead of hanging in the verticals A F and C H, a star I will seem to have the zenith distances *e* I, *g* I, instead of E I, G I, which it ought to have; and the distance F H on the Earth's surface will seem the measure of the difference of latitude *e g*, whereas it corresponds to E G. The measure of a degree, including the space F H, and estimated by the declination of a star I, will be too short, and the measure of a degree, terminating either at F or H, will be too long when the space F H is excluded.

Considerable doubts remaining as to the inferences drawn from this observation, the philosophers were very desirous of having it repeated. For this reason, our Sovereign, George III, ever zealous to promote true science, sent the royal astronomer, Dr Maskelyne, to Scotland, to make this experiment on the north and south sides of Shihallien, a lofty and solid mountain in Perthshire. The deviation toward the mountain on each side exceeded 7″; thus confirming, beyond doubt, the noble discovery of our illustrious countryman.

Perhaps a very sensible effect might be observed at Annapolis-Royal, in Nova Scotia, from the vast addition of matter brought on the coast twice every day by the tides. The water rises there above a hundred feet at spring-tide. If a leaden pipe, a few hundred feet long, were laid on the level beach, at right angles with the coast, and a glass pipe set upright at each end, and the whole filled with water; the water will rise at the outer end, and sink at the end next the land, as the tide rises. Such an alternate change of level would give the most satisfactory evidence. Perhaps the effect might be sensible on a very long plummet, or even a nice spirit level.

236. A very fine and satisfactory examination was made in 1788 by Mr H. Cavendish. Two leaden balls were fastened to the ends of a slender deal rod, which was suspended horizontally at its middle by a fine wire. This arm, after oscillating some time horizontally by the twisting and untwisting of the wire, came to rest in a certain position. Two great masses of lead were now brought within a proper distance of the two suspended balls, and their approach produced a deviation of the arms from the points of rest. By the extent of this deviation, and by the times of the oscillations when the great masses were withdrawn, the proportion was discovered between the elasti-

city of the wire and the gravitation of the balls to the great masses; and a medium of all the observations was taken.

By these experiments, the mutual gravitation of terrestrial matter, even at considerable distances, was most evincingly demonstrated; and it was legitimately deduced from them, that the medium density of the Earth was more than five times the density of water. These curious and valuable experiments are narrated in the Philosophical Transactions for 1798.

237. The oblate form of the Earth also affords another proof that gravity is directed, not to any singular point within the Earth, but that its direction is the combined effect of a gravitation to every particle of matter. Were gravity directed to the centre, by any peculiar virtue of that point, then, as the rotation takes away $\frac{1}{289}$ of the gravity at the equator, the equatorial parts of a fluid sphere must rise one-half of this, or $\frac{1}{578}$, before all is in equilibrio.

For suppose C N and C Q (Fig. 5.) to be two canals reaching from the pole and from the equator to the centre. Since the diminution of gravity at Q is observed to be $\frac{1}{289}$, and the gravitation of every particle in CQ is diminished by rotation in proportion to its distance from the axis of rotation, the diminution occasioned in the weight of the whole canal will be one-half of the diminution it would sustain, if the weight of every particle were as much diminished as that of the particle Q is. Therefore the canal presses less on the centre by $\frac{1}{578}$, and must be lengthened so much before it will balance N C, which sustains no diminution of weight. Every other canal parallel to C Q sustains a similar loss of weight, and must be similarly compensated. This will produce an elliptical spheroidal form.

But the equatorial parts of our globe are much more elevated than this—not less than $\frac{1}{312}$. The reason is this: When the rotation of the Earth has raised the equatorial points $\frac{1}{578}$, the plummet, which at *a*, (Fig. 5.) would have hung in the direction *a* D, tangent to the evolute ABDF, is attracted sideways by the protuberant matter toward the equator. But the surface of the ocean must still be such, that the plummet is perpendicular to it. Therefore it cannot retain the elliptical form produced by the rotation alone, but swells still more at the equator; and this still increases the deviation of the plummet. This must go on, till a new equilibrium is produced by a new figure. This will be considered afterwards. No more is mentioned at present, than what is necessary for shewing, that the protuberance produced by the rotation causes, by its attraction, the plummet to deviate from the position which it had acquired, in consequence of the same rotation.

238. By such induction, and such reasoning, is established the doctrine of universal gravitation, a doctrine which is placed beyond the reach of controversy, and has immortalized the fame of its illustrious inventor.

Sir Isaac Newton has been supposed by many to have assigned this mutual gravitation, or, as he sometimes calls it, this attraction, as a property inherent in matter, and as the *cause* of the celestial phenomena; and for this reason, he has been accused of introducing the occult qualities of the peripatetics into philosophy. Nay, many accuse him of introducing into philosophy a manifest absurdity, namely, that a body can act where it is not present. This, they say, is equivalent with saying, that the Sun attracts the planets, or that any body acts on another that is at a distance from it.

Both of those accusations are unjust. Newton, in no place of that work which contains the doctrine of universal gravitation, that is, in his *Mathematical Principles of Natural Philosophy*, attempts to *explain* the *general* phenomena of the solar system from the principle of universal gravitation. On the contrary, it is in those general phenomena that he discovers it. The only discovery to which he professes to have any claim is, 1*st*, the matter of fact, that every body in the solar system is continually deflected toward every other body in it, and that the deflection of any individual body A toward any other body B, is *observed* to be in the proportion of the quantity of matter in B directly, and of the square of the distance AB inversely; and, 2*dly*, that the falling of terrestrial bodies is just a particular example of this universal deflection. He employs this discovery to explain phenomena that are more particular; and all the explanation that he gives of these is, the shewing that they are modified cases of this general phenomenon, of which he knows no explanation but the mere description. Newton was not more eminent for mathematical genius and penetrating judgment, than for logical accuracy. He uses the word gravitation as the expression, not of a quality, but of a fact; not of a cause, but of an event. Having established this fact beyond the power of controversy, by an induction sufficiently copious, nay, without a single exception, he explains the more particular phenomena, by shewing, with what modifications, arising from the circumstances of the case, they are included in the general fact of mutual deflection; and, *finally*, as all changes of motion are conceived by us as the effects of force, he says, that there is a deflecting force continually acting on every particle of matter in the solar system, and that this deflecting force is what we call weight, heaviness. Few persons think themselves chargeable with absurdity, or with the abetting of occult qualities, when they really consider the heaviness of a body as one of its properties. So far from being occult, it seems one of the most manifest. It is not the heaviness of this body that is the occult quality; it is the cause of this heaviness. In thus considering gravity as competent to all matter, Newton does nothing that is not done by others, when they ascribe impulsiveness or inertia to matter. Without scruple, they say that impulsiveness is an universal property of matter. Impulsiveness and heaviness are on precisely the same footing—mere phenomena; and the most general phenomena that we know. We know none more general than impulsiveness, so as to include it, and thus enable us to explain it. Nor do we know any that includes the phenomena of universal deflection, with all the modifications of the heaviness of matter. Whether one of these can explain the other is a different question, and will be considered on another occasion, when we shall see with how little justice philosophers have refused all action at a distance.

But it would seem that there is some peculiarity in this explanation of the planetary motions, which hinders it from giving entire satisfaction to the mind. If this be the case, it is principally owing to mistake; to carelessly imputing to Newton views which he did not entertain. His doctrine of universal gravitation does not attempt to explain *how* the operating cause retards the Moon's motion in the first and third quarters of a lunation; it merely narrates in what direction, and with what velocity, this change is produced; or rather, it shews how the Moon's deflection toward the Earth, joined to her deflection toward the Sun, both of which are matters of fact, constitute this *seeming* irregularity of motion which we consider as a disturbance. But with respect to the operating cause of this general deflection, and the manner in which it produces its effect, so as to explain that effect, Newton is altogether silent. He was as anxious as any person not to be thought to ascribe inherent gravity to matter, or to assert that a body could act on another at a distance, without some mechanical intervention. In a letter to Dr Bentley, he expresses this anxiety in the strongest terms. It is difficult to know Newton's precise meaning by the word *action*. In very strict language, it is absurd to say that matter acts at all —in contact or at a distance. But if one should assert, that the condition of a particle *a* cannot depend on another particle *b* at a distance from it, hardly any person will say that he makes this assertion from a clear perception of the absurdity of the contrary proposition. Should a person say that the mere presence of the particle *b* is a sufficient reason for *a* approaching it, it will be difficult to prove the assertion to be absurd.

239. Such, however, has been the general opinion of philosophers; and numberless attempts have been made to thrust in some material agent in all the cases of seeming action at a distance. Hence the hypotheses of magnetical and electrical atmospheres; hence the vortexes of Des Cartes, and the celestial machinery of Eudoxus and Callippus.

Of all those attempts, perhaps the most rash and unjustifiable is that of Leibnitz, published in the Leipzig Acts, 1689, two years after the publication of Newton's Principia, and of the review of it in those very acts. It may be called rash, because it trusted too much to the deference which his own countrymen had hitherto shewn for his opinions. In this attempt to account for the elliptical motion of the planets, Leibnitz pays no regard to the acknowledged laws of motion. He assumes as principles of explanation, motions totally repugnant to those laws, and motions and tendencies incongruous and contradictory to each other. And then, by the help of geometrical and analytical errors, which compensate each other, he makes out a strange conclusion, which he calls a demonstration of the law of planetary gravitation; and says, that he sees that this theorem is known to Mr Newton, but that he cannot tell how he has arrived at the knowledge of it. This is something very remarkable. Newton's process is sufficiently pointed out in the *Acta Eruditorum*, which M. Leibnitz acknowledges that he had seen. A copy of the *Principia* was sent to him, by order of the Royal Society, in less than two months after the publication.—It was soon known over all Europe.

It is without the least foundation that the partisans of

M. Leibnitz gave him any share in the discovery of the law of gravitation. None of them has ventured to quote this dissertation as a proposition justly proved, nor to defend it against the objections of Dr Gregory and Dr Keill. M. Leibnitz's remarks on Dr Gregory's criticism were not admitted into the *Acta Eruditorum*, though under the management of his particular friends. In October 1706 they inserted an extract from a letter, containing some of these remarks;—if possible, they are more absurd and incongruous than the original dissertation.

It is worth while, as a piece of amusement, to read the account of this dissertation by Dr Gregory in his Astronomy, and the observations by Dr Keill in the *Journal Litteraire de la Haye*, August 1714.

240. Sir Isaac Newton has also shewn some disposition to account for the planetary deflection by the action of an elastic æther. The general notion of the attempt is this: The space occupied by the solar system is supposed to be filled with an elastic fluid, incomparably more subtle and more elastic than our air. It is supposed to be of greater and greater density as we recede from the Sun, and in general from all bodies. In consequence of this, Newton thinks that a planet placed any where in it will be impelled from a denser into a rarer part of the æther, and in this manner have its course incurvated toward the Sun.

But, without making any remarks on the impossibility of conceiving this operation with any distinctness that can entitle the hypothesis to be called an *explanation*, it need only be observed, that it is, in its first conception, quite unfit for answering the very purpose for which it is employed, namely, to avoid the absurdity of bodies acting on others at a distance. For, unless this be allowed, an æther of different density and elasticity in its different strata cannot exist. It must either be uniformly dense and elastic throughout, or there must exist a repulsive force operating between very distant particles—perhaps extending its influence as far as the solar influence extends—nay, elasticity without an action *è distanti*, even between the adjoining particles, is inconceivable. What is meant by elasticity? Surely such a constitution of the assemblage of particles as makes them recede from each other; and the absurdity is as great at the distance of the millionth part of a hair's breadth as at the distance of a million of leagues. If we attempt to evade this, by saying that the particles are in contact, and are elastic, we must grant that they are compressible, and are really compressed, otherwise they are not exerting any elastic force; therefore they are dimpled, and can no more constitute a fluid than so many blown bladders compressed in a box.

The last attempt of this kind that shall be mentioned, is that of M. Le Sage of Geneva, put into a better shape by M. Prevôt, in a Memoir published by the Academy of Berlin, under the name of *Leucrece Neutonien*. This philosopher supposes that through every point of space there is continually passing a stream of æther in *every* direction, with immense rapidity. This will produce no effect on a solitary body; but if there are two, one of them intercepts part of the stream which would have acted on the other. Therefore the bodies, being less impelled on that side which faces the other, will move toward each other. Le Sage adds some circumstances respecting the structure of the bodies, which may give a sort of progression in the intensity of the impulse, which may produce a deflection diminishing as the distance or its square increases. But this hypothesis also requires that we make light of the acknowledged laws of motion. It has other insuperable difficulties, and, so far from affording any *explanation* of the planetary motions, its most trifling circumstance is incomparably more difficult to comprehend, or even to conceive than the most intricate phenomenon in astronomy.

241. Indeed this difficulty obtains in every attempt of the kind, it being necessary to consider the combined motion of millions of bodies, in order to explain the motion of

one. But such hypotheses have a worse fault than their difficulty; they transgress a great rule of philosophical disquisition, 'never to admit as the cause of a phenomenon any thing of which we do not know the existence.' For, even if the legitimate consequences of the hypothesis were agreeable to the phenomena, this only shews the *possibility* of the theory, but gives no explanation whatever. The hypothesis is good, only as far as it agrees with the phenomena; we therefore understand the phenomena as far as we understand the explanation. The *observed* laws of the phenomena are as extensive as our explanation, and the hypothesis is useless. But, alas! none of those hypotheses agree, in their legitimate consequences, with the phenomena; the laws of motion must be thrown aside, in order to employ them, and new laws must be adopted. This is unwise; it were much better to give those *pro re nata* laws to the planets themselves.

Mr Cotes, a philosopher and geometer of the first eminence, wrote a preface to the second edition of the Principia, which was published in 1713 with many alterations and improvements by the author. In this preface Mr Cotes gives an excellent account of the principles of the Newtonian philosophy, and many very pertinent remarks on the maxim which made philosophers so adverse to the admission of attracting and repelling forces. Whatever may have been Newton's sentiments in early life about the competency of an elastic æther to account for the planetary deflections, he certainly put little value on it afterwards. For he never made any serious use of it for the explanation of any phenomenon susceptible of mathematical discussion. He had certainly rejected all such hypotheses, otherwise he never would have permitted Mr Pemberton to prefix that preface of Mr Cotes to an edition carried on under his own eye. For in this preface the absurdity of the hypothesis of an elastic æther is completely exposed, and it is declared to be a contrivance altogether unworthy of a philosopher. Yet, when Mr Cotes died soon after, Sir Isaac Newton spoke of him in terms of the highest respect. Alas! said he, *we have lost Mr Cotes; had he lived, we should soon have learned something excellent.*

At present the most eminent philosophers and mathematicians in Europe profess the opinion of Mr Cotes, and see no validity in the philosophical maxim that bodies cannot act at a distance. M. de la Place, the excellent commentator of Newton, and who has given the finishing stroke to the universality of the influence of gravitation on the planetary motions, by explaining, by this principle, the secular equation of the Moon, which had resisted the efforts of all the mathematicians, endeavours, on the contrary, to prove that an action in the inverse duplicate ratio of the distances results from the very essence or existence of matter. Some remarks will be made on this attempt of M. de la Place afterwards. But at present we shall find it much more conducive to our purpose to avoid altogether this metaphysical question, and strictly to follow the example of our illustrious Instructor, who clearly saw its absolute insignificance for increasing our knowledge of Nature.

Newton saw that any inquiry into the *manner of acting* of the efficient cause of the planetary deflections was altogether unnecessary for acquiring a complete knowledge of all the phenomena depending on the law which he had so happily discovered. Such was its perfect simplicity, that we wanted nothing but the assurance of its constancy—an assurance established on the exquisite agreement of phenomena with every legitimate deduction from the law.

Even Newton's perspicacious mind did not see the number of important phenomena that were completely explained by it, and he thought that some would be found which required the admission of other principles. But the first mathematicians of Europe have acquired most deserved fame in the cultivation of this philosophy, and in their progress have found that there is not one appearance in the celestial motions that is inconsistent with the Newtonian law, and scarcely a phenomenon that requires any thing else for its complete explanation.

Hitherto we have been employed in the establishment of a general law. We are now to shew how the motions actually observed in the individual members of the solar system result from, or are examples of the operation of the power called Gravity, and how its effects are modified, and made what we behold, by the circumstances of the case.— To do this in detail would occupy many volumes; we must content ourselves with adducing one or two of the most interesting examples. The student in this noble department of mechanical philosophy will derive great assistance from Mr M'Laurin's *Account of Sir Isaac Newton's Dicoveries*. Dr Pemberton's *View of the Newtonian Philosophy* has also considerable merit, and is peculiarly fitted for those who are less habituated to mathematical discussion. The *Cosmographia* of the Abbé Frisi is one of the most valuable works extant on this subject. This author gives a very compendious, yet a clear and perspicuous, account of the Newtonian doctrines, and of all the improvements in the manner of treating them which have resulted from the unremitting labour of the great mathematicians in their assiduous cultivation of the Newtonian philosophy. He follows, in general, the geometrical method, and his geometry is elegant, and yet he exhibits (also with great neatness) all the noted analytical processes by which this philosophy has been brought into its present state.

What now follows may be called an outline of

The Theory of the Celestial Motions.

242. The first general remark that arises from the establishment of universal and mutual gravitation is, that the common centre of the whole system is not affected by it, and is either at rest, or, if in motion, this motion is pro-

duced by a force which is external to the system and acts equally and in the same direction on every body of the system.

243. A force has been discovered pervading the whole system, and determining or regulating the motions of every individual body in it. The problem which naturally offers itself first to our discussion is, to ascertain *what will be the motion of a body, projected from any given point of the solar system, in any particular direction, and with any particular velocity—what will be the form of its path, how will it move in this path, and where will it be at any instant we choose to name?*

Sir Isaac has given, in the 41st proposition of his first book, the solution of this problem, in the most general terms, not limited to the observed law of gravitation, but extended to any conceivable relation between the distances and the intensity of the force. This is, unquestionably, the most sublime problem that can be proposed in mechanical philosophy, and is well known by the name of the INVERSE PROBLEM OF CENTRIPETAL FORCES.

But, in this extent, it is a problem of pure dynamics, and does not make a part of physical astronomy. Our attention is limited to the centripetal force which connects this part of the creation of God—a force inversely proportional to the square of the distances. It may be stated as follows.

Let a body P, (Fig. 23.) which gravitates to the Sun in S, be projected in the direction PN, with the velocity which the gravitation at P to the Sun would generate in it by impelling it along P T, less than P S.

Draw P Q perpendicular to P N. Take P O equal to twice P T, and draw O Q perpendicular to P Q, and Q R perpendicular to P S. Also draw P *s*, making the angle Q P *s* equal to Q P S. Join S Q, and produce S Q till it meet P *s* in *s*.

The body will describe an ellipsis, which P N touches in

P, whose foci are S and *s*, and whose principal parameter is twice P R.

For, draw S N perpendicular to P N. Make P O′ = 2 P O or = 4 P T, and draw O′ Q′ perpendicular to P O′, and describe a circle passing through P, O′ and Q′. It will touch P N, because P O′ Q′ was made a right angle, and therefore P Q′ is the diameter of the circle.

We know that an ellipse may be described by a body influenced by gravitation. This ellipse may have S and *s* for its foci, and P N for a tangent in P, because the angles are equal which P N makes with the two focal lines. This being the case, we know that if P Q, O Q, and Q R, be drawn as directed in the foregoing construction, P O′ Q′ is the circle which has the same curvature with the ellipse in P, whose foci are S and *s*, and tangent P N, and P T is one-fourth of the chord of curvature in P, and P R is half the parameter of the ellipse. Therefore P T is the space along which the body must be uniformly impelled by the force in P, that it may acquire the velocity with which the body, actually describing this ellipse, passes through P. If this body, which we shall call A, thus revolves in an ellipse, we should infer that it is deflected toward S, by a force inversely proportional to the square of its distance from S, and of such magnitude in P, that it would generate the velocity with which the body passes through P^2, by uniformly impelling it along P T.

Now, the other body (which we shall call P) was actually projected in the direction P N, that is, in the direction of A's motion, with the very velocity with which A passes through P in the same direction, and it is under the influence of a force precisely the same that must have influenced A in the same place. The two bodies A and P are therefore in precisely the same mechanical condition; in the same place; moving in the same direction; with the same velocity; deflected by the same intensity of force, acting in the same direction. Their motions in the next moment cannot be different, and they must, at the end of the moment, be again in the same condition; and this must continue. A describes a certain ellipse; P must describe the same; for two motions that are different cannot result from the same force acting in the same circumstances.

This demonstration is given by Sir Isaac Newton in four lines, as a corollary from the proposition in which he deduces the law of planetary deflection from the motion in a conic section. But it seemed necessary here to expand his process of reasoning a little, because the validity of the inference has been denied by Mr John Bernoulli, one of the first mathematicians of that age. He even hinted that Newton had taken that illogical method, because he could not accommodate his 41st proposition to the particular law of gravitation observed in the system. And he claims to himself the honour of having the first demonstrated that a centripetal force, inversely as the square of the distance, necessarily produces a motion in a conic section. The argument by which he supports this bold claim is very singular, coming from a consummate mathematician, who could not be ignorant of its nullity; so that it was not a serious argument, but a trick to catch the uninformed. Newton, says he, might with equal propriety have inferred, from the description of the logarithmic spiral by a body influenced by a force inversely proportional to the cube of the distance, that a body so deflected will describe the logarithmic spiral, whereas we know that it may describe the hyperbolic spiral. Not satisfied with this triumph, he attacks Newton's process in his 41st or general proposition of central forces, saying that it is deduced from principles foreign to the question; and, after all, does not exhibit the body in a state of continued motion, but merely informs us where it will be found, and in what condition, in any assigned moment. He concludes by vaunting his own process as accomplishing all that can be wanting in the problem.

These assertions are the most unfounded and bold vauntings of this vainglorious mathematician; and his own solution is a manifest plagiarism from the writings of Newton, except in the method taken by him to demonstrate the lemma which he as well as Newton premises. Newton's demonstration of this lemma is by the purest principles of free curvilineal motion; and it is, in this respect, a beautiful and original proposition. Bernoulli considers it as synonymous with motion on an inclined plane; with which it has no analogy. The solution of the great problem by Bernoulli is, in every principle, and in every step, the same with Newton's; and the only difference is, that Newton employs a geometrical, and Bernoulli an algebraical expression of the proceeding. Newton exhibits continued motion, whereas Bernoulli employs the differential caculus, which *essentially* exhibits only a succession of points of the path. It is worth the student's while to read Dr Keill's Letter to John Bernoulli, and his examination of this boasted solution of the celebrated problem. But it is still more worth his while to read Newton's solution, and the propositions in M'Laurin's Fluxions and Hermann's Phoronomia, which are immediately connected with this problem. This reading will greatly conduce to the forming a good taste in disquisitions of this kind.*

244. Our occupation at present is much more limited. We are chiefly interested to shew that gravitation produces an elliptical motion, when the space P T, along which the body must be uniformly impelled by the force as it exists in P, in order to acquire the velocity of projection, is less than P S. But every step would have been the same, had we made P T equal to P S (as in Fig. 24.) But we

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* The propositions given by M. de Moivre in No 352. of the *Philosophical Transactions*, and those by Dr Keill in No 317. and 340. are peculiarly simple and good.

should then have found that when the angle Q P *s* is made equal to Q P S, the line P *s* will be parallel to S Q, so that S Q will not intersect it, and the path will not have another focus. It is a parabola, of which P R is the principal parameter.

245. We shall also find that if P T be made greater than P S (as in Fig. 5.) the line P *s* (making the angles Q P S and Q P *s* equal) will cut S Q on the other side of S, so that S and *s* are on the same side of Q. The path will be a hyperbola, of which P R is the principal parameter.

246. This restriction to the conic sections plainly follows from the line P R, the third proportional to P O and P Q, being the principal parameter, whether the path be an ellipse, parabola, hyperbola, or circle.*

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* The only difficulty in the inference of a conic section as the necessary path of a projectile influenced by a force in the inverse duplicate ratio of the distance from the centre, has arisen from the practice of the algebraic analysts, of defining all curve lines by the relation of an abscissa to parallel ordinates. But this is by no means necessary; and all curves which enclose space, are as naturally referable to a focus, and definable by the relation between the radii and a circular arch. An equation expressing the focal chord of curvature is as distinctive as the usual equation, and leads us with ease to the chief properties of the figure. Therefore

Let S P, the *given* distance, be a, and any indeterminate distance be x. Let the perpendicular S N (also given by S P and the given angle S P N) be b, and let p be the perpendicular and q the focal chord of curvature, corresponding to the distance x. Let 4 P T be $= d$. Then we have

$$\frac{1}{b^2 d} : \frac{1}{p^2 q} = \frac{1}{a^2} : \frac{1}{x^2}$$

$$b^2 d : p^2 q = a^2 : x^2$$

$$b^2 d x^2 = p^2 q a^2$$

$$\text{therefore } q = \frac{b^2 d x^2}{a^2 p^2}, = \frac{b^2}{a^2} d \times \frac{x^2}{p^2}$$

Let $\frac{b^2}{a_2} d = e$ then $q = \frac{e x^2}{p^2}$, which is an equation to a conic section, It remains to point out the general circumstances of this elliptical motion, and their physical connexions. For this purpose, the following proposition is useful.

247. When a body describes any curve line B D P A (Fig. 26.) by means of a deflecting force directed to a focus S, the angle S P N, which the radius vector makes with the direction of the motion, diminishes, if the velocity in the point P be less than what would enable the body to describe a circle round S, and increases, if the velocity be greater.

If the velocity of the body in P be less than that which might produce a circular motion round S, then its path will coalesce with the nascent arch P p of a circle whose deflective chord of curvature is less than 2 P S. Let its half be P O, less than P S, and let P p be a very minute arch. Draw the tangents P N, $p\,n$ and the perpendiculars S N, S n. P q perpendicular to P N will meet $p\,q$ perpendicular to $p\,n$ (P p being evanescent) in q the centre of curvature. Draw p S and p O.

It is evident that the angles P qp and P O p are ultimately equal, as they stand on the same arch P p of the equicurve circle, and are, respectively, the doubles of the angles at the circumference. P qp is evidently equal to N S n. Therefore P O p is equal to N S n, and P S p is less than N S n. Therefore P S N is less than p S n, and S P N is greater than S $p\,n$. Therefore the angle S P N diminishes when P O is less than P S, that is, when the velocity in P is less than what would enable the centripetal force in P to retain the body in a circle round S.

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of which e is the parameter, S the focus, and P N a tangent in P. Now e is a given magnitude, because a, b, d, are all given. Expressing the angle S P N by φ, we have $e = d \times \sin.^2 \varphi$. See also for the particular case of a force proportional to $\frac{1}{x^2}$ the dissertations by Dr Keill in the *Phil. Trans.* No 317. and No 340.

On the other hand, if the velocity in P be greater than what suits a circular motion round S, it is plain that P O will be greater than P S, and the angle P S *p* will be greater than N S *n*, and the angle P S N greater than *p* S *n*, and therefore the angle S P N will be less than S *p n*, &c.

248. Applying this observation to the case of elliptical motion, we get a more distinct notion of its different affections, and their dependance on their physical causes.

In the half D A B (Fig. 18.) of the ellipse described by a planet round the Sun in its focus S, the middle point of the deflective or focal chord of curvature lies between the planet and the focus. Therefore, during the whole motion from D to B, along the semiellipse D A B, the angle contained between the radius vector and the line of the planet's motion is continually diminishing. But during the motion in the semiellipse, B P D, the angle is continually increasing. It is therefore the greatest possible in D, and the smallest in B.

Let the planet set out from its aphelion A, with its due velocity, moving in the direction A F. The velocity in A, being equal to that acquired by a uniform acceleration along one-fourth of the parameter, is vastly less than what would make it move in the circular arch A L, of which S is the centre, and the planet must fall within that circle. Therefore its path will no longer be perpendicular to the radius vector, but must now make with it an angle somewhat acute. The centripetal force therefore is now resolvable into two forces, one of which accelerates the planet's motion, and the other incurvates its path. Its direction brings it nearer to the Sun. While in the quadrant A F B, the velocity is always less than what is required for a circular motion. For, if from any point F in this quadrant, F G be drawn perpendicular to the tangent, meeting the transverse axis in G, and if G H be drawn perpendicular to the normal F G, H F is one half of the focal chord of curvature, and H lies between P and S. Now, it has been shewn that when this is the case, the angle S F *n* diminishes, and, with it, the ratio of S *n* to S F (this ratio is that of C B to the semidiameter C O, the conjugate of C F. Consequently, there will be continually more and more of the centripetal force employed in accelerating the motion, and less employed in incurvating the path, the first part being F *n* and the other S *n*. When the planet arrives at B, the point H falls upon S, and the velocity is precisely what would suffice for a circular motion round S, if the direction of the motion were perpendicular to the radius vector. But the direction of the motion brings it still nearer to S. A great part of the centripetal force is still employed in accelerating the motion; and the moment the planet passes B, the velocity becomes greater than what might produce a circular motion round S. For H now lies beyond S from B. Therefore the angle S B N, which is now in its smallest possible state, begins to open again; and this diminishes the proportion of the centripetal force which accelerates the motion, and increases the proportion of the incurvating force. The planet is, however, still accelerated, preserving the equable description of areas. The angle S B N increases with the increasing velocity, and becomes a right angle, when the planet arrives at its perihelion P.

It is shewn, by writers on the conic sections, that the chord P I cut off from any diameter P A by the equicurve circle P *a* I, is equal to the parameter of that diameter. Therefore the centre *o* of this circle lies beyond S. The planet, passing through P, is describing a nascent arch of this circle. Consequently, the curve which it is describing passes without a circle described round S, and the planet is now receding from the Sun. This is usually accounted for, by saying that its velocity is now too great for describing a circle round the Sun. And this is true, when the intensity of the deflecting force is considered. But it has been thought difficult to account for the planet now retiring from the Sun, in the perihelion, where the centripetal force is the greatest of all—greater than what has already been able to bring it continually nearer to the Sun. We are apt to expect that it will come still nearer. But the fact is, that the planet, in passing through P, is really moving, so that, if the Sun were suddenly transferred to *o*, it would circulate round it for ever. But, in describing the smallest portion of the circle P *a* I, it goes without the circle which has S for its centre, and its motion now makes an obtuse angle with the radius vector, although it is perpendicular to a radius drawn to *o*. There is now a portion of the centripetal force employed in retarding the motion of the planet, and its velocity is now diminished; and the angle of the radius vector and the path is now increased, by the same degrees by which they had been increased and diminished during the approach to the Sun. At D, the planet has the same distance from the Sun that it had in B, and the same velocity. The angle S D *v* is now as much greater than a right angle as S B N was less; and at A, it is reduced to a right angle, and the velocity is again the same as the first. In this way the planet will revolve for ever.

It was shewn in Dynamics, that in the curvilineal motion of bodies by the action of a central force, the velocities are inversely as the perpendiculars from the centre of forces on the lines of their directions. In the perihelion, the radius vector is perpendicular to the path. The perihelion distance may therefore be taken as the unit of the scale on which all the other velocities are measured. The other velocities may therefore be considered as fractions of the perihelion velocity, which is the greatest of all.

In elliptical motions, the velocities in every point are as the perpendiculars drawn from the other focus on the tangents in that point. For the perpendiculars on any tangent drawn from the two foci are reciprocal.

249. Hence it appears that if a body sets out from P,

with the velocity acquired by uniform acceleration along P S, and describes a parabola by means of a centripetal force directed to S, the velocity diminishes without limit. For the perpendicular drawn from the focus on a tangent to a parabola may be greater than any line that can be assigned, if the point in the parabola be taken sufficiently remote from the vertex.

250. If the body set out from P with a velocity exceeding what it would acquire by uniform acceleration along P S, it will describe a hyperbola, and its velocity will diminish continually. But it will never be less than a certain determinable magnitude, to which it continually approximates. For the perpendicular from the focus on the tangent in the most remote point of the hyperbola that can be assigned, is still less than the perpendicular to the assymptote, to which the tangent continually approaches.

But, when the velocity in the perihelion is less than that acquired by uniform acceleration along P S, there will always be a limit to its diminution by the recess from the centre of force. For the velocity being so moderate, the path is more incurvated by the centripetal force; so that the body is made to describe a curve which has an upper apsis A, as well as a lower apsis P. The body, after passing through A at right angles to the radius vector, is now accelerated, because its path now makes an acute angle with the radius vector; and thus the velocity is again increased.

251. The velocity in any point of the ellipse described by a planet is to the velocity that would enable the same force to retain it in a circle at the same distance, in the subduplicate ratio of its distance from the upper focus f to the semitransverse axis. That is, calling the elliptic velocity V, and the circular velocity v, we have $V^2 : v^2 = P f : C A$. (Fig. 26.)

For (250.) $V^2 : v^2 = P O : P S$.

But by *conic sections* it is shewn that $P O \times C A$ is equal to $CK^2 = PS \times Ps$. Therefore $PO : PS = Ps : CA$ and $V^2 : v^2 = Ps : CA$.

252. The angular motion in the ellipse is to the angular motion in a circle at the same distance, and by the action of the same force, in the subduplicate ratio of half the parameter to the distance from S.

Take Pp, a small arch of the ellipse, and, with the centre S, and distance SP, describe the circular arch PxV, cutting Sp in x. Make Pp to PV as the velocity in the ellipse to that in the circle. Then it is plain that Px is to PV as the angular motion in the ellipse is to the angular motion in the circle.

The angle xPp being the complement of NPS (because NP may be considered as coinciding with pP) it is equal to NSP. Therefore,

$$
\begin{array}{ll}
Px^2 : Pp^2 = SN^2 : SP^2, & = PQ^2 : PO^2 \\
\text{therefore} & Px^2 : Pp^2 = PR : PO \\
\text{but} & Pp^2 : PV^2 = PO : PS \\
\text{therefore} & Px^2 : PV^2 = PR : PS.
\end{array}
$$

Cor. The angular motion in the circle exceeds that in the ellipse, when the point R lies between P and S, and falls short of it when R lies beyond S. They are equal when PS is perpendicular to AC, or when the true anomaly of the planet is 90°. For then R and S coincide. Here the approach to S is most rapid.

253. In any point of the ellipse, the gravitation or centripetal force is to that which would produce the same angular motion in a circle, at the same distance from the Sun, as this distance is to half the parameter, that is, as PS to PR.

For, by the last proposition, when the forces in the circle and ellipse are the same, the angular motion in the circle was to that in the ellipse as PV to Px, which has been shewn to be as $\sqrt{PS}$ to $\sqrt{PR}$. Therefore, when the angular velocity in the circle, and consequently the real velocity, is changed from PV to Px, in order that it may be the same with that in the ellipse, the centripetal force must be changed in the proportion of PV^2 to Px^2, that is, of PS to PR. Therefore the force which retains the body in the ellipse is to that which will retain it with the same angular motion in a circle at that distance as PS to PR.

These are the chief affections of a motion regulated by a centripetal force in the inverse duplicate ratio of the distance from the centre of forces. The comparison of them with motions in a circle gives us, in most cases, easy means of stating every change of angular motion, or of approach to or recess from the centre, by means of any change of centripetal force, or of velocity.

Such changes frequently occur in the planetary spaces; and the regular elliptical motion of any individual planet, produced by its gravitation to the Sun, is continually disturbed by its gravitation to the other planets. This disturbance is proportional to the square of the distance from the disturbing planet inversely, and to the quantity of matter in that planet directly. Therefore, before we can ascertain the disturbance of the Earth's motion, for example, by the action of Jupiter, we must know the proportion of the quantity of matter in Jupiter to that in the Sun. This may seem a question beyond the reach of human understanding. But the Newtonian philosophy furnishes us with infallible means for deciding it.

Of the Quantity of Matter in the Sun and Planets.

SINCE it appears that the mutual tendency which we have called Gravitation is competent to every particle of matter, and therefore the gravitation of a particle of matter to any mass whatever, is the sum or aggregate of its gravitation to every atom of matter in that mass,—it follows, that the gravitation to the Sun or to a planet is proportional to the quantity of matter in the Sun or the planet. As the gravitation may thus be computed, when we know the quantity of matter, so this may be computed when we know the gravitation towards it. Hence it is evident, that we can ascertain the proportion of the quantities of matter in any two bodies, if we know the proportion of the gravitations toward them.

254. The tendency toward a body, of which m is the quantity of matter, and d the distance, is $\doteqdot \frac{m}{d^2}$. It is this tendency which produces deflection from a straight line, and it is measured by this deflection. Now this, in the case of the planets, is measured by the distance at which the revolution is performed, and the velocity of that revolution. We found, that this combination is expressed by the proportional equation $g \doteqdot \frac{d}{p^2}$, where p is the periodic time. Therefore we have $\frac{m}{d^2} \doteqdot \frac{d}{p^2}$, and, consequently, $m \doteqdot \frac{d^3}{p^2}$.

By this means we can compare the quantity of matter in all such bodies as have others revolving round them. Thus, we may compare the Sun with the Earth, by comparing the Moon's gravitation to the Earth with the Earth's gravitation to the Sun. It will be convenient to consider the Earth as the unit in this comparison with the other bodies of the system.

The Sun's distance in miles is -	93726900
The Moon's distance - -	240144
The Earth's revolution (sidereal) days	365,25
The Moon's sidereal revolution (days) -	27,322

$$\text{Therefore } \frac{93726900^3 \times 27{,}322^2}{240144^3 \times 365{,}25^2} = 332669.$$

But this must be increased by about $\frac{1}{70}$, because the gravitation to the Earth is stated beyond its real value, by the supposition, that the revolution of the Moon is performed round the centre of the Earth, whereas it is really performed round their common centre. Thus increased, the Sun's quantity of matter may be estimated at 337422 times that of this Earth.

It must be observed, that this computation is not of very great accuracy. It depends on the distance of the Sun; and any mistake in this is accompanied by a similar mistake, but in a triplicate proportion. Now our estimation of the Sun's distance depends entirely on the Sun's horizontal parallax, as measured by means of the transits of Venus. The error of $\frac{1}{10}$ of a second in this parallax (which is only about 8″,7 or 8″,8) will induce an error of $\frac{1}{30}$ of the whole.

In like manner, we compare Jupiter with the Earth, by comparing the gravitation of the first satellite with that of the Moon. This makes Jupiter about 313 times more massive than the Earth.

The quantity of matter in Saturn deduced from the revolution of his second Cassinian satellite, is about 108 times that of the Earth.

Herschel's planet contains about 17 times as much matter as our globe, as we learn by the revolution of its first satellite.

We have no such means for obtaining a knowledge of the quantity of matter in Venus, Mars, or Mercury. These are therefore only guessed at, by means of certain physical considerations which afford some data for an opinion. Venus is thought to be about $\frac{19}{20}$ of the Earth, Mars about $\frac{1}{4}$, and Mercury about $\frac{1}{10}$. But these are very vague guesses. We judge of the Moon's quantity of matter with some more confidence, by comparing the influence of the Sun and Moon on the tides, and on the precession of the equinoxes. The Moon is supposed about $\frac{1}{70}$ of the Earth.

From this comparison it will appear, that the Sun contains nearly 800 times as much matter as all the planets combined into one mass. Therefore the gravitation to the

Sun so much exceeds that of any one planet to another, that their mutual disturbances are but inconsiderable.

255. The proportion of the quantities of matter, discovered by this process of reasoning, is very different from what we should have deduced from the observed bulk of the different bodies. Thus, Saturn's diameter being about ten times that of the Earth, we should have inferred, that he contained a thousand times as much matter, whereas he contains only about 103 or 104. We must therefore conclude, that the densities of the Sun and planets are very different. Still taking the Earth as the unit of the scale, and combining the ratios of the bulks and the quantities of matter, we may say, that the density of

The Sun is - -	0,25
Venus - - -	1,27
Earth - - -	1
Mars - - - -	0,73
Jupiter - - -	0,292
Saturn - - - -	0,184
Georgian Planet - -	0,212

It appears, by this statement, that the density of the planets is less, as they are more remote from the centre of revolution. Herschel's planet is an exception; but a small change on his apparent diameter, not exceeding half a second, will perfectly reconcile them.

256. Knowing the quantity of matter, and the diameter of the bodies of the system, we can easily tell the accelerative force of gravity acting on a body at their surfaces by article 465, that is, what velocity gravity will generate in a second of time, or how far a body will fall in a second. In like manner, we can tell the pressure occasioned by the weight or heaviness of a body, as this may be measured by the scale of a spring steelyard, graduated by additions of equal known pressures. It cannot be measured by a balance, which only compares one mass of equally heavy matter with another.

Thus, the space fallen through, and the apparent weight of a lump of matter, by a spring steelyard, will be

	Fall in 1″.	Weight.
At the surface of the Sun -	451 feet.	28,2
Earth -	16,09	1
Jupiter -	41,64	2,6
Saturn -	14,4	0,89
Herschel	18,7	1,16

Of the Mutual Disturbances of the Planetary Motions.

257. The questions which occur in this department of the study are generally of the most delicate nature, and require the most scrupulous attention to a variety of circumstances. It is not enough to know the direction and intensity of the disturbing force in every point of the planet's motion. We must be able to collect into one aggregate the minute and almost imperceptible changes that have accumulated through perhaps a long tract of time, during which the forces are continually changing, both in direction and in intensity, and are frequently combined with other forces. This requires the constant employment of the inverse method of fluxions, which is by far the most difficult department of the higher geometry, and is still in an imperfect state. These problems have been exclusively the employment of the most eminent mathematicians of Europe, the only persons who are in a condition to improve the Newtonian philosophy; and the result of their labours has shewn, in the clearest manner, its supreme excellence, and total dissimilitude to all the physical theories which have occupied the attention of philosophers before the days of the admired inventor. For the seeming anomalies that are observed in the solar system are, all of them, the consequences of the universal operation of one simple force, without the interference of any other, and are all susceptible of the most precise measurement and comparison with

observation; so that what we choose to call anomalies, irregularities, and disturbances, are as much the result of the general pervading principle as the elliptical motions, of which they are regarded as the disturbances.

It is in this part of the study, also, in which the penetrating and inventive genius of Newton appears most conspicuously. The first law of Kepler, the equable description of areas, led the way to all the rest, and made the detection of the law of planetary force a much easier task. But the most discriminating attention was necessary for separating from each other the deviations from simple elliptical motion which result from the mutual gravitation of the planets, and a consummate knowledge of dynamics for computing and summing up all those deviations. The science was yet to create; and it is chiefly to this that the first book of Newton's great work is dedicated. He has given the most beautiful specimen of the investigation in his theory of the lunar inequalities. To every one who has acquired a just taste in mathematical composition, that theory will be considered as one of the most elegant and *pleasing* performances ever exhibited to the public. It is true, that it is but a commencement of a most delicate and difficult investigation, which has been carried to successive degrees of much greater improvement, by the unceasing labours of the first mathematicians. But in Newton's work are to be found all the helps for the prosecution of it, and the first application of his new geometry, contrived on purpose; and all the steps of the process, and the methods of proceeding, are pointed out—all of Newton's invention, *suâ mathesi facem præferente.*

It must be farther remarked, that the knowledge of the anomalies of the planetary motions is of the greatest importance. Without a very advanced state of it, it would have been impossible to construct accurate tables of the lunar motions. But by the application of this theory, Mayer has constructed tables so accurate, that, by observing the distance of the Moon from a properly selected star, the longitude may be found at sea with an exactness quite sufficient for navigation. This method is now universally practised on board of our East India ships. This requires such accurate theory and tables of the Moon's motion, that we must at all times be able to determine her place within the 30th part of her own diameter. Yet the Moon is subject to more anomalies than any other body in the solar system.

But the study is no less valuable to the speculative philosopher. Few things are more pleasing than the being able to trace order and harmony in the midst of seeming confusion and derangement. No where, in the wide range of speculation, is order more completely effected. All the seeming disorder terminates in the detection of a class of subordinate motions, which have regular periods of increase and diminution, never arising to a magnitude that makes any considerable change in the simple elliptical motions; so that, finally, the solar system seems calculated for almost eternal duration, without sustaining any deviation from its present state that will be perceived by any besides astronomers. The display of wisdom, in the selection of this law of mutual action, and in accommodating it to the various circumstances which contribute to this duration and constancy, is surely one of the most engaging objects that can attract the attention of mankind.

In this elementary course of instruction, we cannot give a detail of the mutual disturbances of the planetary motions. Yet there are points, both in respect of doctrine and of method, which may be called elementary, in relation to this particular subject. It is proper to consider these with some attention.

258. The regularity of the motions of a planet A round the Sun would not be disturbed by the gravitation of both to another planet B, if the Sun and the planet A gravitate to B with equal force, and in the same or in a parallel di-

rection. The disturbance arises entirely from the inequality and the obliquity of the gravitations of the Sun and of the planet A to B. The manner in which these disturbances may be considered, and the grounds of computation, will be more clearly understood by an example.

Let S (Fig. 27.) represent the Sun, E the Earth, and J the planet Jupiter. Let it be farther supposed (which may be done without any great error) that the Earth and Jupiter describe concentric circles round the Sun, and that the Sun contains 1000 times as much matter as Jupiter. Make JS to EA as the square of EJ to the square of SJ. Then, if we take SJ to represent the gravitation of the Sun to Jupiter, it is plain that EA will represent the gravitation of the Earth, placed in E, to Jupiter. Draw EB, parallel and equal to JS, and complete the parallelogram EBAD. The force with which Jupiter deranges the motion of the Earth round the Sun will be represented by ED.

For the force EA is equivalent to the combined forces EB and ED. But if the Sun and Earth were impelled only by the equal and parallel forces SJ and EB acting on every particle of each, it is plain, that their relative motions would not be affected (98.) It is only by the impulsion arising from the force ED, that their relative situations will sustain any derangement.

259. This derangement is of two kinds, affecting either the gravitation of the Earth to the Sun, or her angular motion round him. Let ED be considered as the diagonal of a rectangle EFDG, EG lying in the direction of the radius SE, and EF being in the direction of the tangent to the Earth's orbit. It is plain that the force EG affects the Earth's gravitation to the Sun, while EF affects the motion round him. As EG is in the direction of the radius, it has no tendency to accelerate or retard her motion round the Sun. EF, on the other hand, does not affect the gravitation, but the motion in the curve only.

This disturbing force E D varies, both in direction and magnitude, by a variation in the Earth's position in relation to the Sun and Jupiter. Thus, in Fig. A, which represents the Earth as almost arrived at the conjunction with Jupiter, having Jupiter near his opposition to the Sun, the force E G greatly diminishes the Earth's gravitation to the Sun, and the force E F accelerates her motion round him in the order of the letters E C P O Q. In Fig. B, the force E G still diminishes the Earth's gravitation to the Sun, but E F retards her motion from O to Q. In Fig. C, E G increases the Earth's gravitation to the Sun, and E F accelerates her motion round him. It appears very plainly, that the motion round the Sun is accelerated in the quadrants Q C and P O, and is retarded in the quadrants C P and O Q. We may also see, that the gravitation to the Sun is increased in the neighbourhood of the points P and Q, but is diminished in the neighbourhood of C and O, and that there is an intermediate point in each quadrant where the gravitation suffers no change. The greatest diminution of the Earth's gravitation to the Sun must be in C, when Jupiter is nearest to the Earth, in the time of his opposition to the Sun.

We also see, very plainly, how all these disturbing forces may be precisely determined, depending on the proportion of E I to E S and to S I. Nor is the construction restricted to circular orbits. Each orbit is to be considered in its true figure, and the parallelogram E G D F is not always a rectangle, but has the side E F lying in the direction of the tangent. But we believe that the computation is found to be sufficiently exact, without considering the parallelogram E G D F as oblique. The eccentricity of Jupiter's orbit must not be neglected, because it amounts to a fourth part of the Earth's distance from the Sun.

We have taken the Sun's gravitation to Jupiter as the scale on which the disturbing forces are measured; but this was for the greater facility of comparing the disturbing forces with each other. But they must be compared with the Earth's gravitation to the Sun, in order to learn their effect on her motions. It will be exact enough for the present purpose, of merely explaining the method, to suppose Jupiter's mean distance five times the Earth's from the Sun, and that the quantity of matter in the Sun is 1000 times that of Jupiter. Therefore the Earth's gravitation to the Sun must be 25000 times greater than to Jupiter, when the Earth is about P or Q. When the Earth is at C, her gravitation to Jupiter is increased in the proportion of 4^2 to 5^2, and it is now $\frac{1}{16000}$ of her gravitation to the Sun. When the Earth is in O, her gravitation to Jupiter is $\frac{1}{36000}$ of her gravitation to the Sun.

But we are not to imagine, that when the Earth is at C, her motion relative to the Sun is affected in the same manner as if $\frac{1}{16000}$ of her gravitation were taken away. For we must recollect, that the Sun also gravitates to Jupiter, or is deflected toward him, and therefore toward the Earth at C. The diminution of the relative gravitation of the Earth is not to be measured by E A, but by E G. All the disturbing forces, E G and E F, corresponding to every position of the Earth and Jupiter, must be considered as fractions of S J, the measure taken for the mean gravitation to Jupiter. This is $\frac{1}{25000}$ of the Earth's gravitation to the Sun.

Measuring in this way, we shall find, that when the Earth is at P or Q, her gravitation to the Sun is increased by $\frac{1}{125000}$. For P S or Q S will, in this case, come in the place of E G in Fig. C, and there will be no such force as E F. At C, the Earth's gravitation is diminished $\frac{1}{44643}$, and at O, $\frac{1}{31800}$.

To be able to ascertain the magnitude of the disturbing force in the different situations of the Earth, is but a very small part of the task. It only gives us the momentary impulsion. We must ascertain the accumulated effect of the action during a certain time, or along a certain portion of the orbit of the disturbed planet. This is the celebrated *problem of three bodies*, as it is called, which has employed the utmost efforts of the great mathematicians ever since the time that it first appeared in Newton's lunar theory. It can only be solved by approximation; and even this solution, except in some very particular cases, is of the utmost difficulty, which shews, by the way, the folly of all who pretend to *explain* the motions of the planets by the impulsions of fluids, when not three, but millions of particles are acting at once.

We have to ascertain, in the first place, the accumulated effect of the acceleration and retardation of the angular motion of the Earth round the Sun. The general process is one of the two following.

1*st*, Suppose it required to determine how far the attraction of Jupiter has made the Earth overpass the quadrantal arch Q C of her annual orbit. The arch is supposed to be unfolded into a straight line, and divided into minute portions, described in equal times. At each point of division is erected a perpendicular ordinate, equal to the accelerating disturbing force E F corresponding to that point. A curve line is drawn through the extremities of those ordinates. The unfolded arch being considered as the representation of the time, and the ordinates as the accelerating forces, it is plain that the area will represent the acquired velocity. Now, let another figure be constructed having an abscissa to represent the time of the motion. But the ordinates must now be made proportional to the areas of the last figure. It is plain, from a former article, that the area of this new figure will represent, or be proportional to the spaces described, in consequence of the action of the disturbing force; and therefore it will express, nearly, the addition to the space described by the undisturbed planet, or the diminution, if the accelerations have been exceeded by the retardations.

The *other* method is, to make the unfolded arch the space described, and the ordinates the accelerations, as before. The area now represents the augmentation of the square of the velocity. A second figure is now constructed, having the same abscissa now representing the time. The ordinates are made proportional to the square roots of the areas of the first figure, and they will therefore represent the velocities. The areas of this new figure will represent the spaces, as in the first process, to be added to the arch described by the undisturbed planet, or subtracted from it.

260. All this being a task of the utmost labour and difficulty, the ingenuity of the mathematicians has been exercised in facilitating the process. The penetrating eye of Newton perceived a path which seemed to lead directly to the desired point. All the lines which represent the disturbing forces are lines connected with circular arches, and therefore with the circular motion of the planet. The main disturbing force E D is a function of the angle of commutation C S E, and E F and E G are the sine and cosine of the angle D E G. Newton, in his lunar theory, has given most elegant examples of the summation of all the successive lines E F that are drawn to every point of the arch. Sometimes he finds the sums or accumulated actions of the forces expressed by the sine of an arch; sometimes by the tangent; by a segment of the circular area, &c. &c. &c. Euler, D'Alembert, De la Grange, Simpson, and other illustrious cultivators of this philosophy, have immensely improved the methods pointed out and exemplified by Newton, and, by more convenient representations of the forces than this elementary view will admit, have at last made the whole process tolerably easy and plain. But it is still only fit for adepts in the art of symbolical analysis. Their processes are in general so recondite and abstruse, that the analyst loses all conception, either of motions or of forces, and his mind is altogether occupied with the symbols of mathematical reasoning.

261. The second part of the task, the ascertaining the

accumulated effect of the force E G, is, in general, much more difficult. It includes both the changes made on the radius vector S E, and the change made in the curvature of the orbit. The department of mathematical science, immediately subservient to this purpose, is in a more imperfect state than the quadrature of curves. The process is carried on almost entirely by means of converging serieses. We cannot add any thing here that tends to make it plainer. The lunar theory of Newton, with the commentary of Le Seur and Jacquier, commonly called *the Jesuits' Commentary*, gives very good examples of the methods which must be followed in this process. We must refer to the works of Euler, Clairaut, Simpson, and De la Place, on the perturbations of Jupiter and Saturn, &c. and content ourselves with merely pointing out some of the more general and obvious consequences of this mutual action of the planets. La Lande has given in his astronomy a very good synopsis of the most approved method. In the *Tracts, Physical and Mathematical*, by Dr Matthew Stewart, and in his Essay on the Distance of the Sun, are some beautiful specimens of the geometrical solutions of these problems.

262. When we consider the motion of an inferior planet, disturbed by its gravitation to a superior planet, we see that the inferior planet is retarded in the quadrants C P and O Q, and accelerated in the quadrants P O and Q C of its synodical period. Its orbit is more incurvated in the vicinity of the points P and Q, and its curvature is diminished in the vicinity of the points O and C, and most of all in the vicinity of C in the line of conjunction with the superior planet. Therefore, if the aphelion and perihelion of the inferior planet should chance to be near the line J C S O of the synodical motion, these points will seem to shift forward. For, the gravitation of the inferior planet to the Sun being diminished, it will not be able so soon to bend its path to a right angle with the radius vector.

On the other hand, should the apsides of the inferior orbit be near the line P S Q, the increase of the inferior planet's gravitation to the Sun must sooner produce this effect, and it will arrive sooner at its aphelion or perihelion, or those points will seem to come westward and to meet it. And thus, in every synodical revolution, the apsides of the inferior planet will twice advance and twice retreat, as if the elliptical orbit shifted a little to the eastward or westward. But as the diminution of the inferior planet's gravitation to the Sun is much greater when it is in the line C S O than the augmentation of it when in the line P S Q, the advances of the apsides, in the course of a synodical period, will exceed the retreats, and, on the whole, they will advance.

All these derangements, or deviations from the simple elliptical motion, are distinctly observed in the heavens; and the calculated effect on each planet corresponds with what is observed, with all the precision that can be wished for. It is evident that this calculation must be extremely complicated, and that the effect depends not only on the respective positions, but also on the quantities of matter of the different planets. For these reasons, as Jupiter and Saturn are much larger than any of the other planets, these anomalies are chiefly owing to these two planets. The apsides of all the planets are observed to advance, except Venus. It might be imagined, that the vast number of comets, which are almost constantly without the orbits of the planets, would cause a general advance of all the apsides. But these bodies are so far off, and probably contain so little matter, that their action is insensible.

263. The alternate accelerations and retardations of the planets Mercury, Venus, the Earth, and Mars, in consequence of their mutual gravitations, and their gravitations to Jupiter, nearly compensate each other in every revolution; and no effects of them remain after a long tract of time, except an advance of their apsides. But there are peculiarities in the orbits of Jupiter and Saturn, which oc-

casion very sensible accumulations, and have given considerable trouble to the astronomers in discovering their causes. The period of Saturn's revolution round the Sun increases very sensibly, each being about 7 hours longer than the preceding. On the contrary, the period of Jupiter is observed to diminish about half as much, that is, about 1½ hours in each revolution.

This is owing to the particular position of the aphelions of those two planets. Let ABPC (Fig. 28.) be the elliptical orbit of Jupiter, A being the aphelion, and P the perihelion. Suppose the orbit *a b p c* of Saturn to be a circle, having the Sun S in the centre, and let Saturn be supposed to be in *a*. Then, because Jupiter employs more time (about 140 days) in moving from A to C than in moving from C to P, he must retard the motion of Saturn more than he accelerates him, and Jupiter must be more accelerated by Saturn than he is retarded. The contrary must happen if Saturn be in the opposite part *p* of his orbit. After a tract of some revolutions, all must be compensated, because there will be as many oppositions of Saturn to the Sun on one side of the transverse diameter of Jupiter's orbit as on the other.

But if the orbit of Saturn be an ellipse, as in Fig. 28, B, and if the aphelion *a* be 90 degrees more advanced in the order of the signs than the aphelion A of Jupiter, it is plain that there will be more oppositions of Saturn while Jupiter is moving over the semiellipse A C P, than while he moves over the semiellipse P B A, for Saturn is about 400 days longer in the portion *b a c* of his orbit; and therefore Saturn will, on the whole, be retarded, and Jupiter accelerated.

Now, it is a fact, that the aphelion of Saturn is 70 degrees more advanced on the ecliptic than that of Jupiter. Therefore these changes must happen, and the retardations of Saturn must exceed the accelerations. They do so, nearly in the proportion of 353 to 352. This excess will continue for about 2000 years, when the angle A S p will be 90 degrees complete. It will then begin to decrease, and will continue decreasing for 16000 years, after which Saturn will be accelerated, and Jupiter will be retarded. The present retardation of Saturn is about 2′, or a day's motion, in a century, and the concomitant acceleration of Jupiter is about half as much. (See *Mem. Acad. Par.* 1746.)

M. de la Place has happily succeeded in accounting for several irregularities in this gradual change of the mean motions of these two planets, which had considerably perplexed the astronomers in their attempts to ascertain their periods and their maximum by mere observation. These were accompanied by an evident change in the elliptical equations of the orbit, indicating a change of eccentricity. M. de la Place has shewn, that all are precise consequences of universal gravitation, and depend on the *near* equality of five times the angular motion of Saturn to twice that of Jupiter, while the deviation from *perfect* equality of those two motions introduces a variation in these irregularities, which has a very long period (about 877 years). He has at last given an equation, which expresses the motions with such accuracy, that the calculated place agrees with the modern observations, and with the most ancient, without an error exceeding 2′. (See *Mem. Acad. Par.* 1785.)

264. In consequence of the mutual gravitation of the planets, the node of the disturbed planet retreats on the orbit of the disturbing planet. Thus, let E K (Fig. 29.) be the plane of the disturbing planet's orbit, and let A B be the path of the other planet, approaching to the node N. As the disturbing planet is somewhere in the plane E K, its attraction for A tends to make A approach that plane. We may suppose the oblique attraction resolved into two forces, one of which is parallel to E K, and the other perpendicular to it. Let this last be such that, in the time that the planet A, if not disturbed, would move from A to B,

the perpendicular force would cause it to describe the small space A C. By the combined action of this force A C with the motion A B, the planet describes the diagonal A D, and crosses the plane E K in the point *n*. Thus the node has shifted from N to *n*, in a direction contrary to that of the planet's motion. The planet now proceeds in the line *n a*, getting to the other side of the plane E K. The attraction of the disturbing planet now becomes oblique again to the plane, and is partly employed in drawing A (now in *a*) toward the plane. Let this part of the attraction be again represented by a small space *a c*. This, compounded with the progressive motion *a b*, produces a motion in the diagonal *a d*, as if the planet had come, not from *n*, but from N′, a point still more to the westward. The node seems again to have shifted *in antecedentiâ signorum.* And thus it appears that, both in approaching the node, and in quitting the node, the node itself shifts its place, in a direction contrary to that of the motion of the disturbed planet.

It is farther observable, that the inclination of the disturbed orbit increases while the planet approaches the node, and diminishes during the subsequent recess from it. The original inclination A N E becomes A *n* E, which is greater than A N E. The angle A *n* E or *a n* K is afterwards changed into *a*N′K, which is less than *a n* K.

In this manner we perceive that when a planet, having crossed the ecliptic, proceeds on the other side of it, the node recedes, that is, the planet moves as if it had come from a node situated farther west on the ecliptic; and all the while, the inclination of the orbit to the ecliptic is diminishing. When the planet has got 90° eastward from the node which it quitted, it is at the greatest distance from the ecliptic, and, in its farther progress, it approaches the opposite node. Its path now bends more and more *toward* the ecliptic, and the inclination of its orbit to the ecliptic increases, and it crosses the ecliptic again, in a point considerably to the westward of the point where it crossed it before.

The consequence of this modification of the mutual action of the planets is, that the nodes of all their orbits in the ecliptic recede on the ecliptic, except the node of Jupiter's orbit J J (Fig. 30.), which advances on the ecliptic E K, by retreating on the orbit S S of Saturn, from which Jupiter suffers the greatest disturbance.*

265. We have hitherto considered the ecliptic as a permanent circle of the heavens. But it now appears that the Earth must be attracted out of that plane by the other planets. As we refer every phenomenon to the ecliptic by its latitude and longitude in relation to the apparent path of the Sun, it is plain that this deviation of the Sun from a fixed plane, must change the latitude of all the stars. The change is so very small, however, that it never would have been perceived, had it not been pointed out to the astronomers by Newton, as necessarily following from the universal gravitation of matter. The ecliptic (or rather the Sun's path) has a small irregular motion round two points situated about 7½ degrees westward from our equinoctial points.

266. The comets appear to be very greatly deranged in their motions by their gravitation to the planets. The

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* As this motion of the nodes, and that of the apsides formerly mentioned, become sensible by continual accumulation, and as they are equally susceptible of accurate measure and comparison as the greater gravitations which retain the revolving bodies in their orbits, Mr Machin, professor of astronomy at Gresham College, proposed them as the fittest phenomena for informing us of the distance of the Sun. Dr Matthew Stewart made a trial of this method, employing chiefly the motion of the lunar apogee, and has deduced a much greater distance than what can be fairly deduced from the transit of Venus. Notwithstanding some oversights in the summations there given of the disturbing forces, the conclusion seems unexceptionable, and the Sun's distance is, in all probability, not less than 110 or 115 millions of miles.

Halleyan comet has been repeatedly so disturbed by passing near to Jupiter, that its periods were very considerably altered by this action. A comet, observed in 1770 by Lexel, Prosperin, and other accurate astronomers, has been so much deranged in its motions, that its orbit has been totally changed. Its mean distance, period, and perihelion distance, calculated from good observations, which had been continued during three months, agreed with all the observations within 1′ of a degree. In its aphelion, it is a small matter more remote than Jupiter, and must have been so near him in 1767 (about $\frac{1}{80}$ of its distance from the Sun) that its gravitation to Jupiter must have been thrice as great as that to the Sun. Moreover, in its revolution following this appearance in 1770, namely, on the 23d of August 1777, it must have come vastly nearer to Jupiter, and its gravitation to Jupiter must have exceeded its gravitation to the Sun more than 200 times. No wonder then that it has been diverted into quite a different path, and that astronomers cannot tell what is become of it. And this, by the way, suggests some singular and momentous reflections. The number of the comets is certainly great, and their courses are unknown. They may frequently come near the planets. The comet of 1764 has one of its nodes very close to the Earth's orbit, and it is very possible that the Earth and it may chance to be in that part of their respective orbits at the same time. The effect of such vicinity must be very remarkable, probably producing such tides as would destroy most of the habitable surface. But, as its continuance in that great proximity must be very momentary, by reason of its great velocity, the effect may not be so great. When the comet of 1770 was so near to Jupiter, it was *in aphelio*, moving slowly, and therefore may have continued some considerable time there. Yet it does not appear that it produced any derangement in the motion of his satellites. We must therefore conclude, that either the comet did not continue in the path that was supposed, or that it contained only a very small quantity of matter, being perhaps little more than a dense vapour. Many circumstances in the appearance of comets countenance this opinion of their nature. As they retire to very great distances from the Sun, and in that remote situation move very slowly, they may greatly disturb each other's motion. It is therefore a reasonable conjecture of Sir Isaac Newton, that the comet of 1680, at its next approach to the Sun, may really fall into him altogether.

Of the Lunar Inequalities.

267. Of all the heavenly bodies, the Moon has attracted the greatest notice, and her motions have been the most scrupulously examined: and it may be added, that of them all she has been the most refractory. It is but within these few years past that we have been able to ascertain her motions with the precision attained in the cases of the other planets. Not that her apparent path is contorted, like those of Mercury and Venus, running into loops and knots, but because the orbit is continually shifting its place and changing its form; and her real motions in it are accelerated, retarded, and deflected, in a great variety of ways. While the ascertaining the place of Jupiter or Saturn requires the employment of five or six equations, the Moon requires at least forty to attain the *same* exactness. The corrections introduced by those equations are so various, both in their magnitude and in their periods, and have, of consequence, been so blended and complicated together, that it surpassed the power of observation to discover the greatest part of them, because we did not know the occasions which made them necessary, or the physical connexion which they had with the aspects of the other bodies of the solar system. Only such as arose to a conspicuous magnitude, and had an evident relation to the situation of the Sun, were fished out from among the rest.

268. From all this complication and embarrassment the discovery of universal gravitation has freed us. We have only to follow this into its consequences, as modified by the particular situation of the Moon, and we get an equation, which *must* be made, in order to determine a deviation from simple elliptical motion that *must* result from the action of the Sun. This alone, followed regularly into all its consequences, gives all the great equations which the sagacity of observers had discovered, and a multitude of other corrections, which no sagacity could ever have detected.

> Discimus hinc tandem quâ causâ argentea Phœbe
> Passibus haud æquis eat, cur subdita nulli
> Hactenus astronomo, numerorum fræna recusat
> Obvia conspicimus, nubem pollente mathesi.

We have seen that since the Moon accompanies the Earth in its revolution round the Sun, we must conclude that she is under the influence of that force which deflects the Earth into that revolution. If, in every instant, the Moon were impelled by precisely the same force which then impels the Earth, and if this force were also in the same direction, the Moon's motion relative to the Earth would not sustain any change. She would describe an accurate ellipse having the Earth in the focus, and would describe areas proportional to the times. But neither of these conditions are agreeable to the real state of things. The Moon is sometimes nearer to the Sun, and sometimes more remote from him than the Earth is, and is therefore more or less attracted by him; and though the distances of both from the Sun are sometimes equal (as when the Moon is in quadrature) the direction of her gravitation to the Sun is then considerably different from that of the Earth's gravitation to him.

These circumstances change considerably all her motions relative to the Earth. But, since the planetary force follows the precise inverse duplicate ratio of the distances, we can tell what its intensity is in every position of the Moon, in what direction it acts, and what deviation it will produce during any interval of time. We may proceed in the following manner.

269. Let S (Fig. 31.) represent the Sun, E the Earth, moving in the arch A E B. Let the Moon be supposed to describe round the Earth the circle C B O A. Join E S and M S, and let S M cut the Earth's orbit in N. Lastly, Let E S be taken as the measure of the Earth's gravitation to the Sun, and as the scale on which we estimate the disturbing forces.

To learn the magnitude and direction of the force which disturbs the Moon's motion when she is in any point M of her orbit, gravitating to the Sun in the direction M S, we must institute the following analogy $MS^2 : ES^2 = ES : MG$. Then it is evident that if the Moon's gravitation to the Sun be represented by E S when she is in the points A or B, equally distant with the Earth, M G will represent her gravitation to the Sun when she is in M; for it is to E S in the inverse duplicate ratio of the distances from him.

Now this force M G, being neither equal to E S, nor in the same direction, must change or disturb the Moon's motion relative to the Earth. We may suppose M G to result from the combined action of two forces M F and M H (that is, M G may be the diagonal of a parallelogram M F G H), of which one, M F, is parallel and equal to E S. Were the Earth and Moon urged by the forces E S and M F only, their relative motions would not be affected. Therefore M H alone disturbs this relative motion, and may be taken for its indication and measure.

The disturbing force may be otherwise represented by varying the conditions on which the parallelogram M F G H is formed. It may be formed on the supposition that one side of the parallelogram shall have the direction M E. And this is perhaps the best way of resolving M G for the purposes of calculation, and accordingly has been most generally employed by the great geometers who have cultivated this theory. But the method followed in this outline was thought more elementary and most illustrative of the effects.

The magnitude and direction of this disturbing force depends on the form of the parallelogram M F G H and consequently on the proportion of M F and M G, and on their relative positions. We may obtain an easy expression of the force M H by the consideration that the rate of increase of $M S^2$ is double of the rate of increase of M S. When a line increases by a very small addition, the ratio of the increment of the line to the line is but the half of that of the square to the square. Thus, let the line M S be supposed 100, and E S 101, differing by one part in a hundred. We have $M S^2 = 10000$, and $E S^2 = 10201$, differing by very nearly two parts in a hundred; the error of this supposition being only one part in ten thousand. Suppose $M S = 1000$, and $E S = 1001$, differing by one part in a thousand. Then $M S^2 = 1000000$, and $E S = 1002001$, differing from M S by two parts in a thousand very nearly, the error of the supposition being only one part in a million, &c. &c.

Now the greatest difference that can occur between E S and M S is at new and full Moon, when the Moon is in C or O. In this case E C is nearly the 390th part of E S, and we have $E S^2 : O S^2 = 390 : 391^2$, or $= 390 : 392,026$; and therefore, in supposing E S to $O S^2$ as 390 to 392, we commit an error of no more than $\frac{1}{40}$ of $\frac{1}{392}$, that is $\frac{1}{15680}$, viz. less than one part in fifteen thousand, in the most unfavourable circumstances. Therefore the difference between N S (or E S) and M G may be supposed equal to M D, without any sensible error, that is, to the double of N M, the difference of N S and M S. Therefore M G — N S = 2 M N very nearly, and M G — M S, that is, S G = 3 M N very nearly. We may also take M I for M H without any sensible error, and may suppose E I = 3 M N. For the lines M F, I P, H G, being equal and parallel, and

S P nearly coinciding with S G, from which it never deviates more than 9′, E I will nearly coincide with E H, = S G, = 3 M N nearly.

270. These considerations will give us a very simple manner of representing and measuring the disturbing force in every position of the Moon, which will have no error that can be of any significance. Moreover, any error that inheres in it, is completely compensated by an equal error, of an opposite kind, in another point of the orbit. Therefore,

Let us suppose that the portion of the Earth's path round the Sun sensibly coincides with the straight line A B (Fig. 32.) perpendicular to the line O C S, passing through the Sun, and called the line of the SYZIGIES, as A B is called the line of the QUADRATURES. Let M D cross A B at right angles, and produce it to R, so that M P = 3 M N. Join R E, and draw M I parallel to it. M I will, in all cases, have the position and magnitude corresponding to the disturbing force.

Or, more simply, make E I = 3 M N, taking the point I on the same side of A B with M, and draw M I. M I is the disturbing force.

271. This force M I may be resolved into two, viz. M L, having the direction of the Moon's motion, and M K perpendicular to her motion, that is, M K lying in the direction of the radius vector M E, and M L having the direction of the tangent. The force M L affects the Moon's angular motion round the Earth, either accelerating or retarding it, while the force M K either augments or diminishes her gravitation to the Earth.

The disturbing force M I may also be resolved into M R′ = 3 M N, and R′ I, or M E; that is, into a force always proportional to M N, and in that direction, and another force in the direction of the Moon's gravitation to the Earth. This is useful on another occasion.

272. When the Moon is in quadrature, the point I coin-

cides with E, because there is no M N. In this case, therefore, the force M L does not exist, and M K coincides with M E. The disturbing force M I is now wholly employed in augmenting the Moon's gravitation to the Earth. The gravitations of the Earth and Moon to the Sun are equal, but not parallel. If E S expresses the magnitude of the Moon's gravitation to the Sun, then M E will express (on the same scale) the augmentation in quadratures of the Moon's gravitation to the Earth, occasioned by the obliquity of the Sun's action. It is convenient to take this quadrature augment of the Moon's gravitation to the Earth as the unit of the scale on which all the disturbing forces are measured, and to calculate what fraction of her whole gravitation it amounts to.

273. Let G express the Moon's gravitation to the Sun, g her gravitation to the Earth, and g' the increase of this gravitation. Also let y and m be the length of a sydereal year and of a sydereal month. In order to learn in what proportion the Moon's gravitation to the Earth is affected by the disturbing force, it will be convenient to know what proportion its increment in quadrature has to the whole gravitation. We may therefore institute the following proportions.

$$G : g = \frac{D}{P^2} : \frac{d}{p^2} = \frac{ES}{y^2} : \frac{EB}{m^2}\ *$$

$$g' : G = EB : ES. \text{ Therefore}$$

$$g' : g = \frac{ES \times EB}{y^2} : \frac{EB \times ES}{m^2}, = m^2 : y^2.$$

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* $\frac{ES}{y^2} : \frac{EB}{m} = \frac{390}{365{,}256^2} : \frac{1}{27{,}322^2}, = 2{,}1833 : 1$ very nearly. Thus we see that the Moon's gravitation to the Sun is more than twice her gravitation to the Earth. The consequence of this is, that even when the Moon is in conjunction, at new Moon, between the Earth and the Sun, her path in absolute space is concave toward the Sun, and convex toward the Earth. Even there she is deflected, not

The Moon's mean gravitation to the Earth is therefore to its increment in the quadratures by the action of the Sun, in the duplicate ratio of the Earth's period round the Sun to the lunar period round the Earth. This is very nearly in the proportion of 179 to 1. Her gravitation is increased, when in quadrature, about $\frac{1}{179}$. This will diminish the chord of curvature and increase the curvature in the same proportion.

274. In order to see what change it sustains in any other position of the Moon, such as M, join E D, and draw D Q perpendicular to E M. It is plain that D Q is the sine of the angle D E Q, which is twice the angle O E Q or C E M, that is, twice the Moon's distance from the nearest syzigy. Q E is the cosine of the same angle. The triangles MDQ and E I K are similar. E I is equal to 1½ M D. Therefore E K = 1½ M Q, = 1½ M E + 1½ E Q, using the sign + when D E *m* is less than 90°, or C E M is less than 45°, and the sign — when C E M is greater than 45°. Therefore M K = ½ M E + 1½ E Q. Therefore, if ½ M E be equal to 1½ E Q, that is, if M E be = 3 E Q, M K is re-

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toward the Earth, but toward the Sun. This is a very curious, and seemingly paradoxical assertion. But nothing is better established. The tracing the Moon's motion in absolute space is the completest demonstration of it. It is not a looped curve, as one, at first thinking, would imagine, but a line always concave toward the Sun. Indeed scarcely any things can be more unlike than the real motions of the Moon are to what we first imagine them to be. At new Moon, she appears to be moving to the left, and we see her gradually passing the stars, leaving them to the right; and calculating from the distance 240000 miles, and the angular motion, about half a degree in an hour, we should say that she is moving to the left at the rate of 38 miles in a minute. But the fact is, that she is then moving to the right at the rate of 1100 miles in a minute. But as the Earth, from whence we view her, is moving at the rate of 1140 miles in a minute, the Moon is left behind.

duced to nothing, or the force M F is then perpendicular to the radius vector, or is a tangent to the circle. The angle C E M, or the arch C M, has then its secant E I equal to thrice its cosine M N. This arch is 54° 44′. There are therefore four points in the circular orbit distant 54° 44′ from the line of the syzigies, where the Moon's gravitation to the Earth is not affected by the action of the Sun. If the arch C M exceed this, the point K will lie within the orbit, as in Fig. 33. indicating an augmentation of the Moon's gravitation to the Earth.

At B, $1\frac{1}{2}$ E Q = $1\frac{1}{2}$ E M, and therefore $1\frac{1}{2}$ E Q — $\frac{1}{2}$ E M. = E M, as before.

275. At O and at C, $1\frac{1}{2}$ E Q + $\frac{1}{2}$ E M = 2 E M. Therefore, in the syzigies, the diminution of the Moon's gravitation to the Earth is double of the augmentation of it in quadratures, or it is $\frac{1}{89\frac{1}{2}}$ of her gravitation to the Earth.

276. With respect to the force M L, it is evidently = $1\frac{1}{2}$ D Q or $1\frac{1}{2}$ of the sine of twice the Moon's distance from opposition or conjunction. It augments from the syzigy to the octant, where it is a maximum, and from thence it diminishes to nothing in the quadrature. In its maximum state, it is about $\frac{1}{120}$ of the Moon's gravitation to the Earth.

277. It appears, by constructing the figure for the different positions of the Moon in the course of a lunation, that this force M L retards the Moon's motion round the Earth in the first and third quarters C A and O B, but accelerates her motion in the second and last quarters A O and B C. Thus, in Fig. 32, M L leads from M in a direction opposite to that of the Moon's motion eastward from her conjunction at C to her first quadrature in A. In Fig. 34. M L lies in the direction of her motion; and it is plain that M L will be similarly situated in the quadrants C A and O B, as also in the quadrants A O and B C.

All these disturbing forces depend on the proportion of E B to E S. Therefore, while E S remains the same, the disturbing forces will change in the same proportion with the Moon's distance from the Earth.

278. But let us suppose that E S changes in the course of the Earth's motion in her elliptical orbit. Then, did the Sun continue to act with the same force as before, still the disturbing force would change in the proportion of E S, becoming smaller as E S becomes greater, because the proportion of E B to E S becomes smaller. But, when E S increases, the gravitation to the Sun diminishes in the duplicate ratio of E S. Therefore the disturbing force varies in the inverse proportion of E S^3, and, in general, is ÷ $\frac{\text{E B}}{\text{E S}^3}$. Therefore, as the Earth is nearer to the Sun about $\frac{1}{35}$ in January than in July, it follows that in January all the disturbing forces will be nearly $\frac{1}{30}$ greater than in July.

What has now been said must suffice for an account of the forces which disturb the Moon's motion in the different parts of a circular orbit round the Earth. The same forces operate on the Moon revolving in her true elliptical orbit, but varying with the Moon's distance from the Earth. They operate in the same manner, producing, not the same motions, but the same changes of motion.

279. It would seem now that it is not a very difficult matter to compute the motion and the place of the Moon for any particular moment. But it is one of the most difficult problems that have employed the talents of the first mathematicians of Europe. Sir Isaac Newton has treated this subject with his usual superiority, in his Principles of Natural Philosophy, and in the separate Essay on the Lunar Theory. But he only began the subject, and contented himself with marking the principal topics of investigation, pointing out the roads that were to be held in each, and furnishing us with the mathematics and the methods which were to be followed. In all these particulars, great improvements have been made by Euler, D'Alembert,

Clairaut, and Mayer of Gottingen. This last gentleman, by a most sagacious examination and comparison of the *data* furnished by observation, and a judicious employment of the physical principles of Sir Isaac Newton, has constructed equations so exactly fitted to the various circumstances of the case, that he has made his lunar tables correspond with observation, both the most ancient and the most recent, to a degree of exactness that is not exceeded in any tables of the primary planets, and far surpassing any other tables of the lunar motions.

We can, with propriety, only make some very general observations on the effects of the continued action of the disturbing forces.

280. In the syzigies and quadrature, the combined force, arising from the Moon's natural gravitation to the Earth and the Sun's disturbing force, is directed to the Earth. Therefore the Moon will, notwithstanding the disturbing force, continue to describe areas proportional to the times. But as soon as the Moon quits those stations, the tangential force M L begins to operate, and the combined force is no longer directed precisely to the Earth. In the octants, where the tangential force is at its maximum, it causes the combined force to deviate about half a degree from the radius vector, and therefore considerably affects the angular motion.

Let the Moon set out from the second or fourth octant, with her mean angular velocity. Therefore M L, then at its maximum, increases continually this velocity, which augments, till the Moon comes to a syzigy. Here the accelerating force ends, and a retarding force begins to act, and the motion is now retarded by the same degrees by which it was accelerated just before. At the next octant, the sum of the retardations from the syzigy is just equal to the sum of the accelerations from the preceding octant. The velocity of the Moon is now reduced to its mean state. But her place is more advanced by 37′ than it would have been, had the Moon not been affected by the Sun, but had moved from the syzigy with her mean velocity. Proceeding in her course from this octant, the retardation continues, and in the quadrature the velocity is reduced to its lowest state; but here the accelerating force begins again, and restores the velocity to its mean state in the next octant.

Thus it appears, that in the octants the velocity is always in its medium state, attains a maximum in passing through a syzigy, and is the least possible in quadrature. In the first and third octant, the Moon is 37′ east, or a-head of her mean place; and in the second and fourth, is as much to the westward of it; and in the syzigies and quadratures her mean and true places are the same. Thus, when her velocity differs most from its medium state, her calculated and observed places are the same, and where her velocity has attained its mean state, her calculated and observed places differ most widely. This is the case with all astronomical equations. The motions are computed first in their mean state; and when the changing causes increase to a maximum, and then diminish to nothing, the effect, which is a change of place, has attained its maximum by continual addition or deduction.

281. This alternate increase and diminution of the Moon's angular motion in the course of a lunation was first discovered, or at least distinguished from the other irregularities of her motion, by Tycho Brahé, and by him called the Equation of VARIATION. The deduction of it from the principle of universal gravitation by Sir Isaac Newton is the most elegant and perspicuous specimen of mechanical investigation that is to be seen. The address which he has shewn in giving sensible representations and measures of the momentary actions, and of their accumulated results, in all parts of the orbit, are peculiarly pleasing to all persons of a mathematical taste, and are so apposite and plain, that the investigation becomes highly instructive to a beginner in

this part of the higher mathematics. The late Dr Matthew Stewart, in his *Tracts Physical and Mathematical*, following Newton's example, has given some very beautiful examples of the same method.

282. We have hitherto considered the Moon's orbit as circular, and must now inquire whether its form will suffer any change. We may expect that it will, since we see a very great disturbing force diminishing its terrestrial gravity in the syzigies, and increasing it in the quadratures. Let us suppose the Moon to set out from a point 35° 16′ short of a quadrature. The force M K, which we may call a centripetal force, begins to act, increasing the deflecting force. This must render the orbit more incurvated in that part, and this change will be continued through the whole of the arch, extending 35° 16′ on each side of the quadrature. At 35° 16′ east of a quadrature, the gravity recovers its mean state; but the path at this point now makes an acute angle with the radius vector, which brings the Moon nearer to the Earth in passing through the point of conjunction or opposition. Through the whole of the arch V v, extending 54° 44′ on each side of the syzigies, the Moon's gravitation is greatly diminished; and therefore her orbit in this place is flattened, or made less curve than the circle, till at v, 54° 44′ east of the syzigy, the Moon's gravity recovers its mean state, and the orbit its mean curvature.

283. In this manner, the orbit, from being circular, becomes of an oval form, most incurvated at A and B, and least so at O and C, and having its longest diameter lying in the quadratures; not exactly however in those points, on account of the variation of velocity which we have shewn to be greatest in the second and fourth quadrants. The longest diameter lies a small matter short of the points A and B, that is, to the westward of them. Sir Isaac Newton has determined the proportion of the two diameters of this oval, viz. A B = 70 and O C = 69. It may seem strange that

the Moon comes nearest to the Earth when her gravity is most diminished; but this is owing to the incurvation of the orbit in the neighbourhood of the quadratures.

284. The Moon's orbit is not a circle, but an ellipsis, having the Earth in one of the foci. Still, however, the above assertions will apply, by always conceiving a circle described through the Moon's place in the real orbit. But we must now inquire whether this orbit also suffers any change of form by the action of the Sun.

Let us suppose that the line of the apsides coincides with the line of syzigies, and that the Moon is in apogee. Her gravitation to the Earth is diminished in conjunction and opposition, so that, when her gravitation in perigee is compared with her gravitation in apogee, the gravitations differ more than in the inverse duplicate ratio of the distance. The natural forces in perigee and apogee are inversely as the squares of the distance. If the diminutions by the Sun's action were also inversely as the square of the distance, the remaining gravitations would be in the same proportion still. But this is far from being the case here; for the diminutions are directly as the distance, and the greatest quantity is taken from the smallest force. Therefore the forces thus diminished must differ in a greater proportion than before, that is, in a greater ratio than the inverse of the square of the distances.*

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* Thus, let the following perigee and apogee distances be compared, and the corresponding gravitations with their diminutions and remainders.

	Perigee.	*Mean.*	*Apogee.*
Distances - - -	8	10	12
Gravitations - - -	144	100	64
Diminutions - - -	2	2½	3
Remaining gravities -	142	97½	61

Now $12^2 : 8^2 = 142 : 63,11$. Therefore 142 is to 61 in a much greater ratio than the inverse of the square of the distance.

Let the Moon come from the apogee of this disturbed orbit. Did her gravity increase in the due proportion, she would come to the proper perigee. But it increases in a greater proportion, and will bring the Moon nearer to the focus; that is, the orbit will become more eccentric, and its elliptical equation will increase along with the eccentricity. Similar effects will result in the Moon's motion from perigee to apogee. Her apogeum gravity being too much diminished, she will go farther off, and thus the eccentricity and the equation of the orbit will be increased. Suppose the Moon to change when in apogee, and that we calculate her place seven days after, when she should be in the vicinity of the quadrature. We apply her elliptical equation (about 6° 20′) to her mean motion. If we compare this calculation with her real place, we shall find the true place almost 2° behind the calculation. We should find, in like manner, that in the last quadrature, her calculated place, by means of the ordinary equation of the orbit, is more than 2° behind the true or observed place. The orbit has become more eccentric, and the motion in it more unequable, and acquires a greater equation. This may rise to 7° 40′, instead of 6° 20′, which corresponds to the mean form of the orbit.

But let us next suppose, that the apsides of the orbit lie in the quadratures, where the Moon's gravitation to the Earth is increased by the action of the Sun. Were it increased in the inverse duplicate ratio of the distances, the new gravities would still be in this duplicate proportion. But, in the present case, the greatest addition will be made to the smallest force. The apogee and perigee gravities, therefore, will not differ sufficiently; and the Moon, setting out from the apogee in one quadrature, will not, on her arrival at the opposite quadrature, come so near the Earth as she otherwise would have done. Or, should she set out from her perigee in one quadrature, she will not go far enough from the Earth in the opposite quadrature;

that is, the eccentricity of the orbit will, in both cases, be diminished, and, along with it, the equation corresponding. Our calculations for her place in the adjacent opposition or conjunction, made with the ordinary orbital equation, will be faulty, and the errors will be of the opposite kind to the former. The equation necessary in the present case will not exceed 5° 3′.

In all intermediate positions of the apsides, similar anomalies will be observed, verging to the one or the other extreme, according to the position of the line of the apsides. The equation *pro expediendo calculo*, by Dr Halley, contains the corrections which must be made on the equation of the orbit, in order to bring it into the state which corresponds with the present eccentricity of the orbit, depending on the Sun's position in relation to its transverse axis.

285. All these anomalies are distinctly observed, agreeing with the deductions from the effects of universal gravitation with the utmost precision. The anomaly itself was discovered by Ptolemy, and the discovery is the greatest mark of his penetration and sagacity, because it is extremely difficult to find the periods and the changes of this correction, and it had escaped the observation of Hipparchus, and the other eminent astronomers at Alexandria, during three hundred years of continued observation. Ptolemy called it the Equation of EVECTION, because he explained it by a certain shifting of the orbit. His explanation, or rather his hypothesis for directing his calculation, is most ingenious and refined, but is the least compatible with other phenomena of any of Ptolemy's contrivances.

286. The deduction of this anomaly from its physical principles was a far more intricate and difficult task than the variation which equation had furnished. It is, however, accomplished by Newton in the completest manner.

It is an interesting case of the great *problem of three bodies*, which has employed, and continues to employ, the talents and best efforts of the great mathematicians. Mr Machin gave a pretty theorem, which seemed to promise great assistance in the solution of this problem. Newton had demonstrated, that a body, deflected by a centripetal force directed to a fixed point, moved so, that the radius vector described areas proportional to the times. Mr Machin demonstrated, that if deflected by forces directed to two fixed points, the triangle connecting it with them (which may be called the *plana vectrix*) also described solids proportional to the times. Little help has been gotten from it. The equations founded on it, or to which it leads, are of inextricable complexity.

287. Not only the form, but also the position of the lunar orbit, must suffer a change by the action of the Sun. It has already been demonstrated, that if gravity decreased faster than in the proportion of $\frac{1}{d^2}$, the apsides of an orbit will advance, but will retreat, if the gravitation decrease at a slower rate. Now, we have seen, that while the Moon is within 54° 44′ of the syzigies, the gravity is diminished in a greater proportion than that of $\frac{1}{d^2}$. Therefore the apsides which lie in this part of the synodical revolution must advance. For the opposite reasons, while they lie within 35° 16′ of the quadratures, they must recede. But since the diminution in syzigy is double of the augmentation in quadrature, and is continued through a much greater portion of the orbit, the apsides must, in the course of a complete lunation, advance more than they recede, or, on the whole, they must advance. They must advance most, and recede least, when near the syzigies; because at this time the diminution of gravity by the disturbing force bears the greatest proportion to the natural diminution of gravity corresponding to the elliptical motion, and because the augmentation in quadrature will then bear the smallest

proportion to it, because the conjugate axis of the ellipse is in the line of quadrature.

The contrary must happen when the apsides are near the quadratures, and it will be found that, in this case, the recess will exceed the progress. In the octants, the motion of the *apsides in consequentia* is equal to their mean motion; but their place is most distant from their true place, the difference being the accumulated sum of the variations.

But since, in the course of a complete revolution of the Earth and Moon round the Sun, the apsides take every position with respect to the line of the syzigies, they will, on the whole, advance. Their mean progress is about three degrees in each revolution.

288. It has been observed already, that the investigation of the effects of the force M K is much more difficult than that of the effects of the force M L. This last, only treating of acceleration and retardation, rarely employs more than the direct method of fluxions, and the finding of the simpler fluents, which are expressed by circular arches and their concomitant lines. But the very elementary part of this second investigation engages us at once in the study of curvature and the variation of curvature; and its simplest process requires infinite serieses, and the higher orders of fluxions. Sir Isaac Newton has not considered this question in the same systematic manner that he has treated the other, but has generally arrived at his conclusions by more circuitous helps, suggested by circumstances peculiar to the case, and not so capable of a general application. He has not even given us the steps by which he arrived at some of his conclusions. His excellent commentators, Le Seur and Jaquier, have, with much address, supplied us with this information. But all that they have done has been very particular and limited. The determination of the motion of the lunar apogee by the theory of gravity is found to be only one-half of what is really observed. This

was very soon remarked by Mr Machin, but without being able to amend it; and it remained, for many years, a sort of blot on the doctrine of universal gravitation.

289. As the Newtonian mathematics continued to improve by the united labours of the first geniuses of Europe, this investigation received successive improvements also. At last, M. Clairaut, about the year 1743, considered the problem of these bodies, mutually gravitating, in general terms. But finding it beyond the reach of our attainments in geometry, unless considerably limited, he confined his attention to a case which suited the interesting case of the lunar motions. He supposed one of the three bodies immensely larger than the other two, and at a very great distance from them; and the smallest of the others revolving round the third in an ellipse little different from a circle; and limited his attention to the *disturbances only* of this motion.—With this limitation he solved the problem of the lunar theory, and constructed tables of the Moon's motion. But he too found the motion of the apogee only one-half of what is observed.—Euler, and D'Alembert, and Simpson, had the same result; and mathematicians began to suspect that some other force, besides that of a gravitation inversely as the square of the distance, had some share in these motions.

At last, M. Clairaut discovered the source of all their mistakes and their trouble. A term had been omitted, which had a great influence in this particular circumstance, but depended on some of the other anomalies of the Moon, with which he had not suspected any connexion. He found, that the disturbances, which he was considering as relating to the Moon's motion in the simple ellipse, should have been considered as relating to the orbit already affected by the other inequalities. When this was done, he found that the motion of the apogee, deduced from the action of the Sun, was precisely what is observed to obtain. Euler and D'Alembert, who were employed in the same investigation, acceded without scruple to M. Clairaut's improvement of his analysis; and all are now satisfied with respect to the competency of the principle of universal gravitation to the explanation of all these phenomena of the lunar motions.

290. In the whole of the preceding investigation, we have considered the disturbing force of the Sun as acting in the plane of the Moon's orbit, or we have considered that orbit as coinciding with the plane of the ecliptic. But the Moon's orbit is inclined to the plane of the ecliptic nearly 5°, and therefore the Sun is seldom in its plane. His action must generally have a tendency to draw the Moon out of the plane in which she is then moving, and thus to change the inclination of the Moon's orbit to the ecliptic.

But this oblique force may always be resolved into two others, one of which shall be in the plane of the orbit, and the other perpendicular to it. The first will be the disturbing force already considered in all its modifications. We must now consider the effect of the other.*

291. Let A C B O (Fig. 35.) be the Moon's orbit cutting the ecliptic in the line N N′ of the nodes, the half N M A N′ being raised above the ecliptic, and the other half N B O N′ being below it. The dotted circle is the orbit, turned on the line N N′ till it coincide with the plane of the ecliptic. C, O, A, and B, are, as formerly, the points of syzigy and quadrature. Let the Moon be in M. Let A E B be the intersection of a plane perpendicular to

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* It is very difficult to give such a representation of the lunar orbit, inclined to the plane of the ecliptic, that the lines which represent the different affections of the disturbing force may appear detached from the planes of the orbit and ecliptic, and thus enable us to perceive the efficiency of them, and the nature of the effect produced. The most attentive consideration by the reader is necessary for giving him a distinct notion of these circumstances.

the ecliptic. Draw M *n* perpendicular to the plane AEB, and therefore parallel to the ecliptic, and to O C. Take EI equal to 3 M *n*, and join M I. M I is the Sun's disturbing force, (271.) and E M measures the augmentation of the Moon's gravitation when in quadrature. It is plain, that M I is in a plane passing through E S, and intersecting the lunar orbit in the line M E, and the ecliptic in the line E I. M I, therefore, does not lie in the plane of the lunar orbit, nor in that of the ecliptic, but is between them both. The force M I may therefore be conceived as resolvable into two forces, one of which lies in the Moon's orbit, and the other is perpendicular to it. This resolution will be effected, if we draw I *i* upward from the ecliptic, till it meet the plane of the lunar orbit perpendicularly in *i*. Now join M *i*, and complete the parallelogram M*i*I*m*, having M I for its diagonal. The force M I is equivalent to M *i* lying in the plane of the Moon's orbit, and M *m* perpendicular to it. By the force M *i* the Moon is accelerated or retarded, and has her gravitation to the Earth augmented or diminished, while the force M *m* draws the Moon out of the plane N C M; or that plane is made to shift its position, so that its intersection N N′ shifts its place a little. The inclination of the orbit to the ecliptic also is affected. Let a plane I *i* G be drawn through I *i* perpendicular to the line N N′ of the nodes. The line E G is perpendicular to this plane, and therefore to the lines G I and G *i*. Also, I *i* G is a right angle, because I *i* was drawn perpendicular to the plane M *i* G E.

Now, if E M be considered as the radius of the tables, M *n* is the sine of the Moon's distance from quadrature. Call this *q*. Then E I = 3 *q*. Also, making E I radius, I G is the sine of the node's distance from the line of syzigy. Call this *s*. Also, I G being made radius, I *i* or M *m* is the sine of the inclination of the orbit to the ecliptic. Call this *i*.

Therefore we have $EM : EI = R : 3q$

$$EI : IG = R : s$$

$$IG : Mm = R : i$$

Therefore $EM : Mm = R^3 : 3qsi$

and $Mm = 3EM \times \frac{qsi}{R^3}$.

Thus we have obtained an expression of the force M *m*, which tends to change the position and inclination of the orbit. From this expression we may draw several conclusions which indicate its different effects.

Cor. 1. This force vanishes, that is, there is no such force when the Moon is in quadrature. For then *q*, or the line M *n*, is nothing. Now *q* being one of the numerical factors of the numerator of the fraction $\frac{qsi}{R^3}$, the fraction itself has no value. We easily perceive the physical cause of the evanescence of the force M *m* when M comes into the line of quadrature. When this happens, the whole disturbing force has the direction A E, the then radius vector, and is in the plane of the orbit. There is no such force as M *m* in this situation of things, the disturbing force being wholly employed in augmenting the Moon's gravitation to the Earth.

2. The force M *m* vanishes also when the nodes are in the syzigy. For there the factor *s* in the numerator vanishes. We perceive the physical reason of this also. For when the nodes are in the syzigies, the Sun is in the plane of the orbit; or this plane, if produced, passes through the Sun. In such case, the disturbing force is in the plane of the orbit, and can have no part, M *m* acting out of that plane.

3. The chief varieties of the force M *m* depend, however, on *s*, the sine of the node's distance from syzigy. For in every revolution, *q* goes through the same series of successive values, and *i* remains nearly the same in all revolutions. Therefore, the circumstance which will most

distinguish the different lunations is the situation of the node.

292. This force bends the Moon's path *toward* the ecliptic, when the points M and I are on the same side of the line of the nodes, but bends it away *from* the ecliptic when N lies between I and M. This circumstance, kept firmly in mind, and considered with care, will explain all the deviations occasioned by the force M *m*. Thus, in the situation of the nodes represented in the figure, let the Moon set out from conjunction in C, moving in the arch CMAO. All the way from C to A, the disturbing force M I is below the elevated half N M N′ of the Moon's orbit between it and the ecliptic, and therefore the force M *m* pulls the Moon out of the plane of her orbit toward the ecliptic. The same thing happens during the Moon's motion from N to C. This will appear by constructing the same kind of parallelogram on the diagonal M I drawn from any point between N and C.

When the Moon has passed the quadrature A, and is in M′, the force M′ I′ is both above the ecliptic, and above the elevated half of the Moon's orbit. This will appear by drawing M′ *g* perpendicular to E N′, and joining *g* I′. The line M′ *g* is in the orbit, and *g* I′ is in the ecliptic, and the triangle M′ *g* I′ stands elevated, and nearly perpendicular on both planes, so that M′ I′ is above them both. In this case, the force M′ *m*′, in pulling the Moon out of the plane of her orbit, separates her from it on that side which is most remote from the ecliptic; that is, causes the path to approach more obliquely to the ecliptic. The figure 36. will illustrate this. N′ I′ is the ecliptic, and M′ N′ is the orbit, both seen edgeways, as they would appear to an eye placed in *t*, (35.) in the line N N′ produced beyond the orbit. The disturbing force, acting in the direction M′ I′, may be resolved into M′ *p* in the direction of the orbit plane, and M′ *m*′ perpendicular to it. The part M′ *m*′, being compounded with the simultaneous motion M′ q, composes a motion M′ r, which intersects the ecliptic in n. When M′, in Fig. 35, gets to M″, the path is again bent toward the ecliptic, and continues so all the way from N′ to B, where it begins to act in the same manner as in M′ between A and N′.

293. By the action of this lateral force, the orbit must be continually shifting its position, and its intersection with the ecliptic; or, to speak more accurately, the Moon is made to move in a line which does not lie all in one plane. In imagination, we conceive an orbital material line, somewhat like a hoop, of an elliptical shape, all in one plane, passing through the Earth, and, instead of conceiving the Moon to quit this hoop, we suppose the hoop itself to shift its position, so that the arch in which the Moon is, in any moment, takes the direction of the Moon's motion in that moment. Its intersection with the ecliptic (perhaps at a considerable distance from the point occupied by the Moon) shifts accordingly. This hoop may be conceived as having an axis, perpendicular to its plane, passing through the Earth. This axis will incline to one side from the pole of the ecliptic about five degrees, and, as the line NN′ of the nodes shifts round the ecliptic, the extremity of this axis will describe a circle round the pole of the ecliptic, distant from it about 5° all round, just as the axis of the Earth describes a circle round the pole of the ecliptic, distant from it about 23½ degrees.

294. When the Moon's path is bent toward the ecliptic, she must cross it sooner than she would otherwise have done. The node will appear to meet the Moon, that is, to shift to the westward, *in antecedentiâ signorum*, or to recede. But if her path be bent more away from the ecliptic, she must proceed farther before she cross it, and the nodes will shift *in consequentiâ*, that is, will advance.

Cor. 1. Therefore, if the nodes have the situation represented in the figure, in the second and fourth quadrant, the nodes must retreat while the Moon describes the arch

N C A, or the arch N′ O B, that is, while she passes from a node to the next quadrature. But while the Moon describes the arch A N′, or the arch B N, the force which pulls the Moon from the plane of the orbit, causes her to pass the points N′ or N before she reach the ecliptic, and the node therefore advances, while the Moon moves from quadrature to a node.

It is plain, that the contrary must happen when the nodes are situated in the first and third quadrants. They will advance while the Moon proceeds from a node to the next quadrature, and recede while she proceeds from a quadrature to the next node.

Cor. 2. In each synodical revolution of the Moon, the nodes, on the whole, retreat. For, to take the example represented in the figure, all the while that the Moon moves from N to A, the line M I lies between the orbit and ecliptic, and the path is continually inclining more and more towards it, and, consequently, the nodes are all this while receding. They advance while the Moon moves from A to N′. They retreat while she moves from N′ to B, and advance while she proceeds from B′ to N. The time, therefore, during which the nodes recede, exceeds that during which they advance. There will be the same difference or excess of the regress of the nodes when they are situated in the angle C E A.

It is evident that the excess of the arch N C A above the arch B N or A N′, is double of the distance N C of the node from syzigy. Therefore the retreat or westerly motion of the nodes will gradually increase as they pass from syzigy to quadrature, and again decrease as the node passes from quadrature to the syzigy.

Cor. 3. When the nodes are in the quadratures, the lateral force M m is the greatest possible through the whole revolution, because the factor s in the formula $\frac{q\,s\,i}{r^3}$ is then equal to radius. In the syzigies it is nothing.

The nodes make a complete revolution in 6803^d 2^h 55′ 18″, but with great inequality, as appears from what has been said in the preceding paragraphs. The exact determination of their motions is to be seen in Newton's Principia, B. III. Prop. 32.; and it is a very beautiful example of dynamical analysis. The principal equation amounts to 1° 37′ 45″ at its maximum, and, in other situations, it is proportional to the sine of twice the arch N C. The annual regress, computed according to the principles of the theory, does not differ two minutes of a degree from what is actually observed in the heavens. This wonderful coincidence is the great boast of the doctrine of universal gravitation. At the same time, the perusal of Newton's investigation will shew that such agreement is not the *obvious* result of the happy simplicity of the great regulating power; we shall there see many abstruse and delicate circumstances, which must be considered and taken into the account before we can obtain a true statement.

This motion of the nodes is accompanied by a variation of the inclination of the orbit to the ecliptic. The inclination increases, when the Moon is drawn from the ecliptic while leaving a node, or toward it in approaching a node. It is diminished, when the Moon is drawn toward the ecliptic when leaving a node, or from it in approaching a node. Therefore, when the nodes are situated in the first and third quadrants, the inclination increases while the Moon passes from a node to the next quadrature, but it diminishes till she is 90° from the node, and then increases till she reaches the other node. Therefore, in each revolution, the inclination is increased, and becomes continually greater, while the node recedes from the quadrature to the syzigy; and it is the greatest possible when the nodes are in the line of the syzigies, and it is then nearly 5° 18′ 30″. When the nodes are situated in the second and fourth quadrants, the inclination of the orbit diminishes while the Moon passes from the node to the 90th degree ; it is increased from thence to the quadrature, and then diminishes till the Moon reaches the other node. While the nodes are thus situated, the inclination diminishes in every revolution, and is the least of all when the node is in quadrature, and the Moon in syzigy, being then nearly 4°. 58′, and it gradually increases again till the nodes reach the line of syzigy. While the nodes are in the quadratures, or in the syzigies, the inclination is not sensibly changed during that revolution.

Such are the general effects of the lateral force M *m*, that appear on a slight consideration of the circumstances of the case. A more particular account of them cannot be given in this outline of the science. We may just add, that the deductions from the general principle agree precisely with observation. The mathematical investigation not only points out the periods of the different inequalities, and their relation to the respective positions of the Sun and Moon, but also determines the absolute magnitude to which each of them rises. The only quantity deduced from mere observation is the mean inclination of the Moon's orbit. The time of the complete revolution of the nodes, and the magnitude and law of variation of this motion, and the change of inclination, with all its varieties, are deduced from the theory of universal gravitation.

295. There is another case of this problem, which is considerably different, namely, the satellites of Dr Herschel's planet, the planes of whose orbits are nearly perpendicular to the orbit of the planet. This problem offers some curious cases, which deserve the attention of the mechanician ; but as they interest us merely as objects of curiosity, they have not yet been considered.

296. There is still another considerable derangement of the lunar motions by the action of the Sun. We have seen, that in quadrature the Moon's gravitation to the Earth is augmented $\frac{1}{179}$, and that in syzigy it is diminished $\frac{2}{179}$. Taking the whole synodical revolution to-

gether, this is equivalent, nearly, to a diminution of $\frac{\frac{1}{2}}{179}$, or $\frac{1}{358}$. That is to say, in consequence of the Sun's action, the general gravitation of the Moon to the Earth is $\frac{1}{358}$ less than if the Sun were away. If the Sun were away, therefore, the Moon's gravitation would be $\frac{1}{358}$ greater than her present mean gravitation. The consequence would be, that the Moon would come nearer to the Earth. As this would be done without any change on her velocity, and as she now will be retained in a smaller orbit, she will describe it in a proportionally less time; and we can compute exactly how near she would come before this increased gravitation will be balanced by the velocity We must conclude from this, that the mean distance and the mean period of the Moon which we observe, are greater than her natural distance and period.

From this it is plain, that if any thing shall increase or diminish the action of the Sun, it must equally increase or diminish the distance which the Moon assumes from the Earth, and the time of her revolution at that distance.

Now, there actually is such a change in the Sun's action. When the Earth is *in perihelio*, in the beginning of January, she is nearer the Sun than in July by 1 part in 30; consequently, the ratio of E M to E S is increased by $\frac{1}{30}$, or in the ratio of 30 to 31. But her gravitation (and consequently the Moon's) to the Sun is increased $\frac{1}{15}$, or in the ratio of 30 to 32. Therefore the disturbing force is increased by 1 part in 10 nearly. The Moon must therefore retire farther from the Earth 1 part in 1790. She must describe a larger orbit, and employ a greater time.

We can compute exactly what is the extent of this change. The sidereal period of the Moon is $27^d\ 7^h\ 43'$, or 39343′. This must be increased $\frac{1}{1790}$, because the Moon retains the same velocity in the enlarged orbit, This will make the period 39365′, which exceeds the other 22′. The observed difference between a lunation in January and one in July somewhat exceeds 25′. This, when reduced in the proportion of the synodical to the periodical revolution, agrees with this mechanical conclusion with great exactness, when the computation is made with due attention to every circumstance that can affect the conclusion. For it must be remarked, that the computation here given proceeds on the legitimacy of assuming a general diminution of $\frac{1}{358}$ of the Moon's gravitation as equivalent to the variable change of gravity that really takes place. In the particular circumstances of the case, this is very nearly exact. The true method is to take the average of all the disturbing forces M K through the quadrant, multiplying each by the time of its action. And here Euler makes a sagacious remark, that if the diameter of the Moon's orbit had exceeded its present magnitude in a very considerable proportion, it would scarcely have been possible to assign the period in which she would have revolved round the Earth; and the greatest part of the methods by which the problem has been solved could not have been employed.

297. There still remains an anomaly of the lunar motions that has greatly puzzled the cultivators of physical astronomy. Dr Halley, when comparing the ancient Chaldean observations with those of modern times, in order to obtain an accurate measure of the period of the Moon's revolution, found, that some observations made by the Arabian astronomers, in the eighth and ninth centuries, did not agree with this measure. When the lunar period was deduced from a comparison of the Chaldean observations with the Arabian, the period was sensibly greater than what was deduced from a comparison of the Arabian and the modern observations; so that the Moon's mean motion seems to have accelerated a little. This conclusion was confirmed by breaking each of these long intervals into parts. When the Chaldean and Alexandrian observations were compared, they gave a longer period than the Alexandrian compared with the Arabian of the eighth century;

and this last period exceeded what is deduced from a comparison of the Arabian with the modern observations; and even the comparison of the modern observations with each other shews a continued diminution. This conjecture was received by the mechanical philosophers with hesitation, because no reason could be assigned for the acceleration; and the more that the Newtonian philosophy has been cultivated, the more confidently did it appear that the mean distances and periods could sustain no change from the mutual action of the planets. Nay, M. de la Grange has at last demonstrated, that in the solar system, as it exists, this is strictly true, as to any change that will be permanent: all is periodical and compensatory. Yet, as observation also improved, this acceleration of the Moon's mean motion became undeniable and conspicuous, and it is now admitted by every astronomer, at the rate of about 11" in a century, and her change of longitude increases in the duplicate ratio of the times.

Various attempts have been made to account for this acceleration. It was imagined by several, that it was owing to the resistance of the celestial spaces, which, by diminishing the progressive velocity of the Moon, caused her to fall within her preceding orbit, approaching the Earth continually in a sort of elliptical spiral. But the free motion of the tails of comets, the rare matter of which seems to meet with no sensible resistance, rendered this explanation unsatisfactory. Others were disposed to think, that gravity did not operate instantaneously through the whole extent of its influence. The application of this principle did not seem to be obvious, nor its effects to be very clear or definite.

At last, M. de la Place discovered the cause of this perplexing fact; and in a dissertation read to the Royal Academy of Sciences in 1785, he shews, that the acceleration of the Moon's mean motion necessarily arises from a small change in the eccentricity of the Earth's orbit round the

Sun, which is now diminishing, and will continue to diminish for many centuries, by the mutual gravitation of the planets. He was led to the discovery by observing, in the series which expresses the increase of the lunar period by the disturbing force of the Sun (a series formed of sines and cosines of the Moon's angular motion and their multiples), a term equal to $\frac{1}{119}$ of her angular motion multiplied by the square of the eccentricity of the Earth's orbit. Consequently, when this eccentricity becomes smaller, the natural period of the Moon is less enlarged by the Sun's action, and therefore, if the Earth's eccentricity continue to diminish, so will the lunar period, and this in a duplicate proportion. Without entering into the discussion of this analysis, which is abundantly complicated, we may see the general effect of a diminution of the Earth's eccentricity in this manner. The ratio of the cube of the mean distance of the Earth from the Sun to the cube of her perihelion distance, is greater than the ratio of the cube of her aphelion distance to that of the mean distance. Hence it follows, that the increase of the mean lunar period, during the smaller distances of the Earth from the Sun, is greater than its diminution during her greater distances; and the sum of all the lunations, during a complete revolution of the Earth, exceeds the sum of the lunations that would have happened in the same time, had the Earth remained at her mean distance from the Sun. Therefore, as the Earth's eccentricity diminishes, the lunar period also diminishes, approximating more and more to her period, undisturbed by the change in the Sun's action. M. de la Place finds the diminution in a century = 11″,135, which differs little from that assumed by Mayer from a comparison of observations. This centurial change of angular velocity must produce a change in the space described, that is, in the Moon's longitude, in the duplicate proportion of the time, as in any uniformly accelerated motion. Therefore 11″,135, multiplied by the square of the number of centuries forward or backward, will give the correction of the Moon's longitude computed by the present tables. La Place finds, that, in going back to the Chaldean observations, we must employ another term (nearly $\frac{1}{25}$ of a second) multiplied by the cube of the number of centuries. With these corrections, the computation of the Moon's place agrees with all observations, ancient and modern, with most wonderful accuracy; so that there no longer remains any phenomenon in the system which is not deducible from the Newtonian gravitation.

298. We should, before concluding this account of the perturbations of the planetary motions, pay some attention to the motions of the other secondary planets, and particularly of Jupiter's satellites, seeing that the exact knowledge of their motions is almost as conducive to the improvement of navigation and geography as that of the lunar motions. But there is no room for this discussion, and we must refer to the dissertations of Wargentin, Prosperin, La Place, and others, who have studied the operation of physical causes on those little planets with great assiduity and judgment, and with the greatest success. The little system of Jupiter and his satellites has been of immense service to the philosophical study of the whole solar system. Their motions are so rapid, that, in the course of a few years, many synodical periods are accomplished, in which the perturbations arising from their mutual actions return again in the same order. Nay, such synodical periods have been observed as bring the whole system again into the same relative situation of its different bodies. And, in cases where this is not *accurately* accomplished, the deficiency introduces a small difference between the perturbations of any period and the corresponding perturbations of the preceding one; by which means another and much longer period is indicated, in which this difference goes through all its varieties, swelling to a maximum, and again diminishing to nothing. Thus

the system of Jupiter and his satellites, as a sort of epitome of the great solar system, has suggested to the sagacious philosopher the proper way of studying the great system, namely, by *looking out* for similar periods in *its* anomalies, and by boldly asserting the reality of such corresponding equations as can be shewn to result from the operation of universal gravitation. The fact is, that we have now the most demonstrative knowledge of many such periods and equations, which could not be deduced from the observations of many thousand years.

In the course of this investigation, M. de la Grange has made an important observation, which he has demonstrated in the most incontrovertible manner, namely, that it *necessarily* results from the small eccentricity of the planetary orbits—their small inclination to each other—the immense bulk of the Sun—and from the planets all moving in one direction—that all the perturbations that are observed, nay *all that can exist* in this system, are periodical, and are compensated in opposite points of every period. He shews also that the greatest perturbations are so moderate, that none but an astronomer will observe any difference between this perturbed state and the mean state of the system. The mean distances and the mean periods remain for ever the same. In short, the whole assemblage will continue, almost to eternity, in a state fit for its present purposes, and not distinguishable from its present state, except by the prying eye of an astronomer.

Cold, we think, must be the heart that is not affected by this mark of beneficent wisdom in the Contriver of the magnificent fabric, so manifest in selecting for its connecting principle a power so admirably fitted for continuing to answer the purposes of its first formation. And he must be little susceptible of moral impression who does not feel himself highly obliged to the Being who has made him capable of perceiving this display of wisdom, and has attached to this perception sentiments so pleasing and delightful. The extreme simplicity of the constitution of the solar system is perhaps the most remarkable feature of its beauty. To this circumstance are we indebted for the pleasure afforded by the contemplation. For it is this alone that has allowed our limited understanding to acquire such a comprehensive body of well-founded knowledge, far exceeding, both in extent and in accuracy, any thing attained in other paths of philosophical research. But we have not yet seen all the capabilities of this wonderful power of nature. Let us therefore still follow our excellent leader in a new path of investigation.

Of the Figures of the Planets.

299. Sir Isaac Newton, having so happily explained all the phenomena of *progressive* motion exhibited by the heavenly bodies, by shewing that they are all, without exception, modified examples of deflection towards one another, in the inverse duplicate ratio of the distances, was induced to examine the other motions observed in some of those bodies, to see what modification these motions received by the influence of universal gravitation. The Sun, and several planets, turn round their axis. The study of celestial mechanism is not complete, till we see whether this kind of motion is in any way influenced by gravitation.

It does not appear, at first consideration, that there can be any great mystery in the mere rotation of a body round its axis. It seems to be one of the simplest mechanical questions. But the fact is just the opposite. Before the rotative motion that we observe in our Earth can be secured, in the way in which we see it actually performed, adjustments are necessary, which are very abstruse, and required all the sagacity of Newton to discover and appreciate; and it is acknowledged that this is the department of physical astronomy where his acuteness of discernment appears the most remarkable. It is also the class of phenomena in which the effects of universal gravitation are most convincingly seen. For this reason, some more notice will be taken of the rotation of the planets, and of its consequences, than is usually done in our elementary treatises. But, as in the other departments, so here, it is only the more simple and general facts that can be considered. To go a very small step beyond these, engages us at once in the most difficult problems, which have occupied and still occupy the first mathematicians of Europe, and require all the resources of their science. Such discussion, however, would be unsuitable here. But without some attempt of this kind, we must remain ignorant of the mechanism of some phenomena, more familiar and important than many of those which we have already discussed.

When a body turns round an axis, each particle describes a circle, to which this axis is perpendicular. Now we know that a particle of matter cannot describe a circle, unless some deflecting force retain it in the periphery. In coherent masses, this retaining force is supplied by the cohesion. But even this is a limited thing. A stone may be so briskly whirled about in a sling, that the cord will break. Grindstones are sometimes whirled about in our manufactures with such rapidity that they split, and the pieces fly off with prodigious force. If matters be lying loose on the surface of a revolving planet, their gravitation may be insufficient to retain them in that velocity of rotation. In every case, the force which actually retains such loose bodies on the surface can be found only in their weight; and part of it is thus expended, and they continue to press the ground only with the remainder. If the velocity of rotation be increased to a certain degree, it may require the whole weight of the body for its supply. If the velocity still increase, the body is not retained, but thrown off. If this Earth turn round in 84 minutes, things lying on the equator might remain there; but they would not press the ground, nor stretch the thread of a plummet. For this is precisely the time in which a planet would circulate round the Earth, close to the surface, moving about 17 times faster than a cannon ball. The weight of the body, deflecting it 16 feet in a second, just keeps it in the circumference of a circle close to the surface of the Earth. The Earth, turning as fast, will have the planet always immediately above the same point of its surface; and the planet will not appear to have any weight, because it will not descend, but keep hovering over the same spot. If the rotation were still swifter, every thing would be thrown off, as we see water flirted from a mop briskly whirled round.

300. As things are really adjusted, this does not happen. But yet there is a certain measureable part of the weight of any body expended in keeping it at rest, in the place where it lies loose. At the equator, a body lying on the ground describes, in one second, an arch of 1528 feet nearly. This deviates from the tangent nearly $\frac{67}{100}$ of an inch. This is very nearly $\frac{1}{288}$ part of $16\frac{1}{12}$ feet, the space through which gravity, or its heaviness, would cause a stone to fall in that time. Hence we must infer that the centrifugal tendency arising from rotation is $\frac{1}{288}$ of the sensible weight of a body on the equator, and $\frac{1}{289}$ of its real weight. Were this body therefore taken to the pole, it would manifest a greater heaviness. If, at the equator, it drew out the scale of a spring steelyard to the division 288, it would draw it to 289 at the pole.

301. M. Richer, a French mathematician, going to Cayenne in 1672, was directed to make some astronomical observations there, and was provided with a pendulum clock for this purpose. He found that his clock, which had been carefully adjusted to mean time at Paris, lost above two minutes every day, and he was obliged to shorten the pendulum $\frac{1}{10}$ of an inch before it kept right time. Hence he concluded, that a heavy body dropped at Cayenne would not fall 193 inches in a second. It would fall only about $192\frac{1}{3}$. Richer immediately wrote an account of this

very singular diminution of gravity. It was scouted by almost all the philosophers of Europe, but has been confirmed by many repetitions of the experiment. Here then is a direct proof that the heaviness of a body, whether considered as a mere pressure, or as an accelerating force, is employed, and in part expended, in keeping bodies united to a whirling planet.

302. These considerations are not new. Even in ancient times, men of reflection entertained such thoughts. The celebrated Roman general Polybius, one of the most intelligent philosophers of antiquity, is quoted by Strabo, as saying, that in consequence of the Earth's rotation, every body was made lighter, and that the globe itself swelled out in the middle. Were it not so, says he, the waters of the ocean would all run to the shores of the torrid zone, and leave the polar regions dry. Dr Hooke is the first modern philosopher who professed this opinion. Mr Huyghens, however, is the first who gave it the proper attention. Occupied at the time of Richer's remark with his pendulum clocks, he took great interest in this observation at Cayenne, and instantly perceived the true cause of the retardation of Richer's clock. He perceived that pendulums must vibrate more slowly, in proportion as their situation removes them farther from the axis of the Earth; and he assigned the proportion of the retardation in different places.

303. Resuming this subject some time after, it occurred to him, that unless the Earth be protuberant all around the equator, the ocean must overflow the lands, increasing in depth till the height of the water compensated for its diminished gravity. He considers the condition of the water in a canal reaching from the surface of the equator to the centre of the Earth (suppose the canal C Q, Fig. 5.) and there communicating with a canal C N reaching from the centre to the pole. The water in the last must retain all its natural gravity, because its particles do not describe circles round the axis. But every particle in the

column C Q reaching to the surface of the equator must have its weight diminished in proportion to its distance from the centre of the globe. Therefore the whole diminution will be the same as if each particle lost half as much as the outermost particle loses. This is very plain. Therefore these two columns cannot balance each other at the centre, unless the equatoreal column be longer than the polar column by $\frac{1}{578}$ (for the extremity of this column loses $\frac{1}{289}$ of its weight by the centrifugal force employed in the rotation.)

Being an excellent and zealous geometer, this subject seemed to merit his serious study, and he investigated the form that the ocean must acquire so as to be *in equilibrio*. This he did by inquiring what will be the position of a plummet in any latitude. This he knew must be perpendicular to the surface of still water. On the supposition of gravity directed to the centre of the Earth, and equal at all distances from that centre, he constructed the meridional curve, which should in every point have the tangent perpendicular to the direction of a plummet determined by him on these principles.

304. At this very time, another circumstance gave a peculiar interest to this question of the figure of the Earth. The magnificent project of measuring the whole arch of the meridian which passes through France was then carrying on. It seemed to result from the comparison of the lengths of the different portions of this arch, that the degrees increased as they were more southerly. This made the academicians employed in the measurement conclude that the Earth was of an egg-like shape. This was quite incompatible with the reasoning of Mr Huyghens. The contest was carried on for a long while with great pertinacity, and some of the first mathematicians of the age abetted the opinion of those astronomers, and the honour of France was made a party in the dispute. The opinion of Mr Huyghens, the greatest ornament of

their academy, could not prevail; indeed his inferences were such, in some respects, that even the impartial mathematicians were dissatisfied with them. The form which he assigned to the meridian was very remarkable, consisting of two paraboloidal curves, which had their vertex in the poles, and their branches intersected each other at the equator, there forming an angular ridge, elevated about seven miles above the inscribed sphere. No such ridge had been observed by the navigators of that age, who had often crossed the equator. Nor had any person on shore at the line observed that two plummets near each other were not parallel, but sensibly approached each other. All this was unlike the ordinary gradations of nature, in which we observe nothing abrupt.

305. While this question was so keenly agitated in France, Mr Newton was engaged in the speculations which have immortalized his name, and it was to him an interesting thing to know what form of a whirling planet was compatible with an equilibrium of all the forces which act on its parts. He therefore took the question up in its most simple form. He supposed the planet completely fluid, and therefore every particle is at liberty to change its place, if it be not in perfect equilibrium. The particles all attract one another with a force in the inverse duplicate ratio of the distance, and they are at the same time actuated by a centrifugal tendency, in consequence of the rotation; or, to express it more accurately, part of those mutual attractions is employed in keeping the particles in their different circles of rotation. He demonstrated that this was possible, if the globe have the form of an elliptical spheroid, compressed at the poles, and protuberant at the equator $\frac{1}{231}$ part of the axis. He also pointed out the phenomena by which this may be ascertained, namely, the variation of gravity as we recede from the equator to the poles, shewing that the increments of sensible gravity are as the squares of the sines of the latitude. This can easily be decided by

experiments with nice pendulum clocks. He shewed also that the remaining gravity, on different parts of the Earth's surface, is inversely proportional to the distance from the centre, when estimated in the direction of the centre, &c. &c. His demonstration of the precise elliptical form consists in proving two things: 1st, That on this supposition, gravity is always perpendicular to the surface of the spheroid: 2d, That all rectilineal canals leading from the centre to the surface will balance one another. Therefore the ocean will maintain its form.

It was some time before the philosophy of Newton could prevail in France over the hypothesis of the French philosopher Des Cartes; and the great mathematician Bernoulli endeavoured to shew that the oblong form of the Earth which had been demonstrated (he says) by the measurement of the degrees, was the effect of the pressure of the vortices in which the Earth was carried about.

306. Mr Hermann, a mathematician of most respectable talents, took another view of the question of the figure of the Earth. Newton had demonstrated, in the most convincing manner, that particles gravitated to the centre of similar solids, or portions of a solid, with forces proportional to their distances from the centre. Hermann availed himself of this, and of another theorem of Newton founded on it, viz. that superficial gravity in different latitudes is inversely as the distance from the centre. But he observed that Newton had by no means demonstrated the elliptical form, but had merely assumed it, or, as it were, guessed at it. This is indeed true, and his application is made by means of the vulgar rule of false position. Hermann, therefore, set himself to inquire what form a fluid will assume when turning round an axis, its particles situated in the same diameter gravitating to the centre proportionally to their distance, yet exhibiting a superficial gravity in different parts inversely as the distance from the centre. He found it to be an ellipse, with such a protuberancy,

that the radius of equator is to the semiaxis in the subduplicate ratio of the primitive equatoreal gravity to the remaining equatoreal gravity. This gives the same proportion of the axes which had been assigned by Huyghens, though accompanied by a very different form. He then inverted his process, and demonstrated the perpendicularity of gravity to the surface, the equilibrium of canals, and some other conditions that appeared indispensable; and he found all right. This confirmed him in his theory, and he found fault with Dr D. Gregory, the commentator of Newton, for adhering to Newton's form of the ellipse. He defied them to point out any fault in his own demonstration of the elliptical figure, and considered this as sufficient for proving the inaccuracy of the Newtonian conjecture, for it could get no higher name.

307. By very slow degrees, the French academicians began to acknowledge the compressed form of the Earth, and to re-examine their observations, by which it had seemed that the degrees increased to the southward. They now affected to find that their measurement had been good, but that some circumstances had been overlooked in the calculations, which should have been taken into the account. But they were not aware that they were now vindicating the goodness of their instruments, and of their eyesight at the expence of their judgment.

All these things made the problem of the figure of the Earth extremely interesting to the great mathematical philosophers. Newton took no part in the further discussion, being satisfied with the evidence which he had for his own determination of the precise species of the terraqueous spheroid. His philosophy gradually acquired the ascendancy; but the comparison made of the degrees of the meridian, argued a smaller ellipticity than he had assigned to the Earth, on the supposition of uniform density and primitive fluidity. He had, however, sufficiently pointed out the varieties of ellipticity which might arise from a difference of density in the interior parts. These were acquiesced in, and the mathematicians speculated on the ways by which the observations and the theory of universal gravitation might be adapted to each other. But, all this while, the original problem was considered as too difficult to be treated in any case remarkably deviating from a sphere; and even this case was solved by Newton and his followers only in an indirect manner.

308. The first person who attempted a direct general solution, was Mr James Stirling. In 1735, he communicated to the Royal Society of London two elegant propositions, (but without demonstration) which determine the form of a homogeneous spheroid turning round its axis, and which, when applied to the particular case of the Earth, perfectly coincided with Newton's determination. In 1737, Mr Clairaut communicated to our Royal Society, and also to the Royal Academy at Paris, very elaborate and elegant performances on the same subject, which he afterwards enlarged in a separate publication. This is the completest work on the subject, and is full of the most curious and valuable research, in which are discussed all the circumstances which can affect the question. It is also remarkable for an example of candour, very rare among rivals in literary fame. The author, in extending his memoir to a more complete work, quits his own method of investigation, though remarkable for its perspicuity and neatness, for that of another mathematician, because it was superior; and this with unaffected acknowledgment of its superiority. The results of Clairaut's theory, perfectly coincide with the Newtonian theory, making the equatoreal diameter to the polar diameter as 231 to 230, though it is agreed, by all the mathematicians, that Newton's method had a chance of being inaccurate. So true is the saying of Daniel Bernoulli, when treating this subject in his theory of the tides, "*The sagacity of that great man* (*Newton*) "*saw clearly through a mist what others can scarcely discover through a microscope.*"

Mr Stirling had said, that the revolving figure was not an accurate elliptical spheroid, but approached infinitely near to it. Mr Clairaut's solutions, in most cases, suppose the spheroid very nearly a sphere, or suppose lines and angles equal which are only very nearly so. Without this allowance, the treatment of the problem seemed impracticable. This made Mr Stirling's assertion more credited; and we apprehend that it became the general opinion, that the solutions obtainable in our present state of mathematical knowledge were only approximations, exact indeed, to any degree that we please, in the cases exhibited in the figures of the planets, but still they were but approximations.

309. But, in 1740, Mr M'Laurin, in a dissertation on the tides, which shared the prize given by the Academy of Paris, demonstrated, in all the rigour and elegance of ancient geometry, that an homogeneous elliptical spheroid, of *any eccentricity whatever*, if turning in a proper time round its axis, will for ever preserve its form. He gave the rule for investigating this form, and the ratio of its axes. His final propositions to this purpose, are the same that Mr Stirling had communicated without demonstration. This performance was much admired, and settled all doubts about the figure of a homogeneous spheroid turning round its axis. It is indeed equally remarkable for its simplicity, its perspicuity, and its elegance. Mr M'Laurin had no occasion to prosecute the subject beyond this simple case. Proceeding on his fundamental propositions, the mathematical philosophers have made many important additions to the theory. But it still presents many questions of most difficult solution, yet intimately connected with the phenomena of the solar system.

In this elementary outline of physical astronomy, we cannot discuss those things in detail. But it would be a capital defect not to include the *general* theory of the figure of planets which turn round their axes. No more, however, will be attempted than to shew that a homogeneous elliptical spheroid will answer all the conditions that are required, and to give a *general* notion of the change which a variable density will produce in this figure.*

The following lemma, from Mr M'Laurin, must be premised.

310. Let $AEBQ$ and $aebq$ (Fig. 37.) be two concentric and similar ellipses, having their shorter axes AB and ab coinciding. Let PaL touch the interior ellipse in the extremity a of the shorter axis, to which let PK, a chord of the exterior ellipse be parallel, and therefore equal. Let the chords af and ag of the interior ellipse make equal angles with the axis, and join their extremities by the chord fg perpendicular to it in i. Draw PF and PG parallel to af and ag, and draw FH and PI perpendicular to PK.

Then, PF together with PG are equal to twice ai, when PF and PG lie on different sides of PK. But if they are on the same side (as PF' and PG') then zai is equal to the difference of PF' and PG'.

Draw Kk parallel to PG or ag, and therefore equal to PF, being equally inclined to KP. Draw the diameter MCz, bisecting the ordinates Kk, PG, and ag, in m, s, and z, and cutting PK in n.

By similarity of triangles, we have

$$Km : Kn = Ps : Pn, = az : aC, = ag : ab,$$

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* The student will consult, with advantage, the original dissertations of Mr Clairaut and Mr M'Laurin, and the great additions made by the last in his valuable work on Fluxions. The *Cosmographia of Frisius,* also contains a very excellent epitome of all that has been done before his time; and the *Mechanique Celeste* of *La Place,* contains some very curious and recondite additions. A work of *F. Boscovich,* on the figure of the Earth, has peculiar merit. This author, by employing geometrical expressions of the acting forces, wherever it can be done, gives us very clear ideas of the subject.

Therefore $Km + Ps : Kn + Pn = ag : ab$,
and Kk (or PF) $+ PG : 2PK = 2ag : 2ab$,
and $PF + PG : 2ag = 2PK : 2ab$;
and, by similarity of triangles, we have

$$PH + PI : 2ai = 2PK : 2ab.$$

But, $2PK = 2ab$. Therefore $PH + PI = 2ai$, and $PI' - PH' = 2ai'$.

311. Let the two planes A G g B (Fig. 38.) A E e B, intersecting in the line A B, and containing a very small angle G A E, be supposed to comprehend a thin elementary wedge or slice of a solid consisting of gravitating matter. If two planes G P E, F P D, standing perpendicularly on the plane A D d B, contain a very small angle EPD, they will comprehend a slender or elementary pyramid of this slice, having its vertex in P, and a quadrilateral base GEDF. If two other planes $g p e$, $f p d$, be drawn from another point p, respectively parallel to the planes G P E, $f p d$, they will comprehend another pyramid, having its sides parallel to those of the other, and containing equal angles, and the elementary pyramids F P E, $f p e$, may therefore be considered as similar. The base $g e d f$ is not indeed always parallel and similar to G E D F. But for each of them may be substituted spherical surfaces, having their centres in P and in p, and then they will be similar.

The gravitation of a particle P to the pyramid G P D is to the gravitation of p to the pyramid $g p d$ as any side P D of the one to the homologous side $p d$ of the other. This is evident by what has already been shewn.

The same proportion will hold when the absolute gravitation in the direction of the axis of the pyramid is estimated in any other direction, such as P m. For drawing $p n$ parallel to P m, and the perpendiculars D m, $d n$, it is plain that the ratio $PD : pd = Pm : pn, = Dm : dn$.

This proposition is of most extensive use. For we thus estimate the gravitation of a particle to any solid, by resolving it into elementary pyramids; and having found the gravitation to each, and reduced them all to one direction, the aggregate of the reduced forces is the whole gravitation of the particle estimated in that direction. The application of this is greatly expedited by the following theorem.

312. Two particles similarly situated in respect of similar solids, that is to say, situated in similar points of homologous lines, have their whole gravitations proportional to any homologous lines of the solids.

For, we can draw through the two particles straight lines similarly posited in respect of the solids, and then draw planes passing through those lines, and through similar points of the solids. The sections of the solids made by those two planes must be similar, for they are similarly placed in similar solids. We can then draw other planes through the same two straight lines, containing with the former planes very small equal angles. The sections of these two planes will also be similar, and there will be comprehended between them and the two former planes similar slices of the two solids.

We can now divide the slices into two serieses of similar pyramids, by drawing planes such as G P E, *g p e*, and F P D, *f p d*, of Fig. 38. the points P and *p* being supposed in different lines, related to each of the two solids. By the reasonings employed in the last proposition, it appears that, when the whole of each slice is occupied by such pyramids, the gravitations to the corresponding pyramids are all in one proportion. Therefore, the gravitation compounded of them all is in the same proportion. As the whole of each of the two similar slices may be thus occupied by serieses of similar and similarly situated pyramids, so the whole of each of the two similar solids may be occupied by similar slices, consisting of such pyramids. And as the compound gravitations to those slices are similarly formed, they are not only in the proportion of the homologous lines of the solids, but they are also in similar directions. Therefore, finally, the gravitations compounded of these compound gravitations are similarly compounded, and are in the same proportion as any homologous lines of the solids.

These things being premised, we proceed to consider the particular case of elliptical spheroids.

313. Let A E B Q, *a e b q* (Fig. 37.) be concentric and similar ellipses, which, by rotation round their shorter axis A *a b* B, generate similar concentric spheroids. We may notice the following particulars.

314. (*a*) A particle *r*, on the surface of the interior spheroid, has no tendency to move in any direction in consequence of its gravitation to the matter contained between the surfaces of the exterior and interior spheroids. For, drawing through *r* the straight line P *r t* G, it is an ordinate to some diameter C M, which bisects it in *s*. The part *r t* comprehended by the interior spheroid is also an ordinate to the same diameter, and is bisected in *s*. Therefore P *r* is equal to *t* G. Now, *r* may be conceived as at the vertex of two similar cones or pyramids, on the common axis P *r* G. By what was demonstrated in art. 224. and 311, it appears, that the gravitation of *r* to the matter of the cone or pyramid, whose axis is *r* P, is equal and opposite to the gravitation to the matter contained in the *frustum* of the similar cone or pyramid, whose axis is *t* G. As this is true, in whatever direction P *r* G be drawn through *r*, it follows, that *r* is *in equilibrio* in every direction, or it has no tendency to move in any direction.

315. (*b*) The gravitations of two particles, P and *p*, (Fig. 39.) situated in one diameter P C, are proportional to their distances P C, *p* C, from the centre. For the gravitation of *p* is the same as if all the matter between the surfaces A E B Q and *a e b q* were away (by the last article), and thus P and *p* are similarly situated on similar solids; and P C and *p* C are homologous lines of those solids; and the proposition is true, by sect. 312.

316. (*c*) All particles equally distant from the plane of the equator gravitate towards that plane with equal forces.

Let P be the particle, (Fig. 37.) and P *a* a line perpendicular to the axis, and parallel to the equator E Q. Let P *d* be perpendicular to the equator. Let *a e b q* be the section of a concentric and similar spheroid, having its axis *a b* coinciding with A B. Drawing any ordinate *f g* to the diameter *ab* of the interior ellipse, join *a f* and *a g*, and draw P F and P G parallel to *a f* and *a g*, and therefore making equal angles with P *d* K. Let *f g* cut *a b* in *i*, and draw F H, G I, perpendicular to P I.

The lines P F and P G may be considered as the axes of two very slender pyramids, comprehended between the plane of the figure and another plane intersecting it in the line P *a* L, and making with it a very minute angle. These pyramids are constituted according to the conditions described in art. 311. The lines *a f*, *a g*, are, in like manner, the axes of two pyramids, whose sides are parallel to those of P F and P G. The gravitation of P to the matter contained in the pyramids P F and P G, and the gravitation of *a* to the pyramids *a f* and *a g*, are as the lines P F, P G, *a f*, and *a g*, respectively. These gravitations, estimated in the direction P *d*, *a* C, perpendicular to the equator, are as the lines P H, P I, *a i*, *a i*, respectively. Now it has been shewn, (310.) that P H + P I are equal to *a i* + *a i*. Therefore the gravitations of P to this pair of pyramids, when estimated perpendicularly to the equator, is equal to the gravitation of *a* to the corresponding pyramids lying on the interior ellipse *a e b q*.

It is evident, that by carrying the ordinate *f g* along the whole diameter from *b* to *a*, the lines *a f*, *a g*, will diverge more and more (always equally) from *a b*, and the pyramids of which these lines are the axes, will thus occupy the whole surface of the interior ellipse. And the pyramids on the axes P F and P G, will, in like manner, occupy the whole of the exterior ellipse. It is also evident, that the whole gravitation of P, estimated in the direction P *d*, arising from the combined gravitations to every pair of pyramids estimated in the same direction, is equal to the whole gravitation of *a*, arising from the combined gravitation to every corresponding pair of pyramids. That is, the gravitation of P in the direction P *d* to the whole of the matter contained in the elementary slice of the spheroid comprehended between the two planes which intersect in the line P *a* L, is equal to the gravitation of *a* to the matter contained in that part of the same slice which lies within the interior spheroid.

But this is not confined to that slice which has the ellipse A E B Q for one of its bounding planes. Let the spheroid be cut by any other plane passing through the line P *a* L. It is known that this section also is an ellipse, and that it is concentric with, and similar to the ellipse formed by the intersection of this plane with the interior spheroid *a e b q*. They are concentric similar ellipses, although not similar to the generating ellipses A E B Q and *a e b q*. Upon this section may another slice be formed by means of another section through P *a* L, a little more oblique to the generating ellipse A E B Q. And the solidity of this section may, in like manner, be occupied by pyramids constituted according to the conditions mentioned in art. 312.

From what has been demonstrated, it appears that the gravitation of P to the whole matter of this slice, estimated in the direction perpendicular to P *a* L, is equal to the gravitation of *a* to the matter in the portion of this slice contained in the interior spheroid.

Hence it follows, that when these slices are taken in every direction through the line P *a* L, they will occupy the whole spheroid; and that the gravitation of P to the matter in the whole solid, estimated perpendicularly to P *a* L, is equal to the gravitation of *a* to the matter that is contained in the interior spheroid, estimated in the same manner.

This gravitation will certainly be in the direction perpendicular to the plane of the equator of the two spheroids. For the slices which compose the solid, all passing through the generating ellipse A E B Q, may be taken in pairs, each pair consisting of equal and similar slices, equally inclined to the plane of the generating ellipse. The gravitations to each slice of a pair are equal, and equally inclined to the plane A E B Q. Therefore they compose a gravitation in the direction which bisects the angle contained by the slices, that is, in the direction of the plane A E B Q, and parallel to its axis A B, or perpendicular to the equator.

From all this it follows, that the gravitation of P to the whole spheroid, when estimated in the direction P *d* perpendicular to the plane of its equator, is equal to the gravitation of *a* to the interior spheroid *a e b q*, which is evidently in the same direction, being directed to the centre C.

In like manner, the gravitation of another particle P′ (in the line P *a* L), in a direction perpendicular to the equator of the spheroid, is equal to the gravitation of *a* to the interior spheroid *a e b q;* for P′ may be conceived as on the surface of a concentric and similar spheroid. When thus situated, it is not affected by the matter in the spheroidal stratum without it, and therefore its gravitation is to be estimated in the same way with that of the particle P. Consequently, the gravitation of P and of P′, estimated in a direction perpendicular to the equator, are equal, each being equal to the central gravitation of *a* to the spheroid *a e b q*. Therefore, all particles equidistant from the equator gravitate equally toward it.

317. (*d*) By reasoning in the same manner, we prove, that the gravitation of a particle P in the direction P *a*, perpendicular to the axis A B, is equal to the gravitation

of the particle *d* to the concentric similar spheroid *d* α *q* β; and therefore all particles equidistant from the axis gravitate equally in a direction perpendicular to it.

318. (*e*) The gravitation of a particle to the spheroid, estimated in a direction perpendicular to the equator, or perpendicular to the axis, is proportional to its distance from the equator, or from the axis. For the gravitation of P in the direction P *d* is equal to the gravitation of *a* to the spheroid *a e b q*. But the gravitation of *a* to the spheroid *a e b q*, is to the gravitation of A to A E B Q as *a* C to A C (312.) Therefore the gravitation of P in the direction P *d* is to the gravitation of A to the spheroid A E B Q as *a* C to A C, or as P *d* to A C; and the same may be proved of any other particle. The gravitation of A is to the gravitation of any particle, as the distance A C is to the distance of that particle. All particles, therefore, gravitate towards the equator proportionally to their distances from it.

In the same manner it is demonstrated, that the gravitation of E to the spheroid in the direction E C perpendicular to the axis, is to the gravitation of any particle P in the same direction as E C to P *a*, the distance of that particle from the axis.

Therefore, &c.

319. (*f*) We are now able to ascertain the direction and intensity of the compound or absolute gravitation of any particle P.

For this purpose, let A represent the gravitation of the particle A in the pole, and E the gravitation of a particle E on the surface of the equator; also, let the force with which P is urged in the direction P *d* be expressed by the symbol *f*, P *d*, and let *f*, P *a* express its tendency in the direction P *a*. We have,

$$f,\ Pd : A = Pd : AC$$

and $$A : E = A : E$$

and $$E : f,\ Pa = EC : Pa.$$ Therefore

$$f,\ Pd : f,\ Pa = Pd \times A \times EC : AC \times E \times Pa.$$

Now, make $d\,C : d\,v = A \times E\,C : E \times A\,C$, and draw $P\,v$. We have now f, $P\,d : f$, $P\,a = P\,d \times d\,C : P\,a \times d\,v$, $= P\,d \times P\,a : P\,a \times d\,v$, $= P\,d : d\,v$. P is therefore urged by two forces, in the directions $P\,d$ and $P\,a$, and these forces are in the proportion of $P\,d$ and $d\,v$. Therefore the compound force has the direction $P\,v$.

Moreover, this compound force is to the gravity at the pole, or the gravitation of the particle A, as $P\,v$ to A C. For the force $P\,v$ is to the force $P\,d$ as $P\,v$ to $P\,d$; and the force $P\,d$ is to A as $P\,d$ to AC. Therefore the force $P\,v$ is to A as $P\,v$ to A C.

In like manner, it may be compared with the force at E. Make $a\,C : a\,u = E \times C\,A : A \times C\,E$. We shall then have f, $P\,a : f$, $P\,d = P\,a : a\,u$; and the force in the direction $P\,a$, when compounded with that in the direction $P\,d$, form a force in the direction $P\,u$, and having to the force at E the proportion of $P\,u$ to E C.

Thus have we obtained the direction of gravitation for any individual particle on the surface, and its magnitude when compared with the forces at A and at E, which are supposed known.

320. (g) But it is necessary to have the measure of the accumulated force or pressure occasioned by the gravitation of a column or row of particles.

Draw the tangent E T, and take any portion of it, such as E T, to represent the gravitation of the particle E. Join C T, cutting the perpendicular $d\,\delta$ in δ. Since the gravitations of particles in one diameter are as their distances from the centre (315.) $d\,\delta$ will express the gravitation of a particle d. Thus, the gravitation of the whole column E C will be represented by the area of the triangle C E T, and the gravitation of the part E d, or the pressure exerted by it at d, is represented by the area $E\,T\,\delta\,d$. We may also conveniently express the pressure of the column E C at C by $\frac{E \times E\,C}{2}$, and in like manner, $\frac{A \times A\,C}{2}$ expresses the weight of the column AC, or the pressure exerted by at C.

Should we express the gravitation of E by a line ET equal to EC, the weight of the whole column EC would be expressed by $\frac{EC^2}{2}$, and that of the portion Ed by $\frac{EC^2 - dC^2}{2}$, or by its equal $\frac{Ed \times dQ}{2}$. We see, also, that whatever value we assign to the force E, the gravitations or pressures of the columns EC and Ed are proportional to EC^2, and $EC^2 - dC^2$, or to EC^2 and $Ed \times dQ$. This remark will be frequently referred to.

321. From these observations it appears, that the two columns AC and EC will exert equal or unequal pressures at the centre C, according to the adjustment of the forces in the direction of the axis, and perpendicular to the axis. If the ellipse do not turn round an axis, then, in order that the fluid in the columns AC and EC may press equally at C, we must have $A \times AC = E \times EC$, or $AC : EC = E : A$. The gravitation at the pole must be to that at the equator, as the radius of the equator to the semiaxis. But we shall find, on examination, that such a proportion of the gravitations at A and E cannot result solely from the mutual gravitation of the particles of a homogeneous spheroid, and that this spheroid, if fluid, and at rest, cannot preserve its form.

322. The six preceding articles ascertain the mechanical state of a particle placed any where in a homogeneous spheroid, inasmuch as it is affected solely by the mutual gravitation to all the other particles. We are now to inquire what conditions of form and gravitating force will produce an exact equilibrium in every particle of an elliptical spheroid of gravitating fluid when turning round its axis. For this purpose, it is necessary, in the first place, that the direction of gravity, affected by the centrifugal force of rotation, be every where perpendicular to the sur-

face of the spheroid, otherwise the waters would flow off toward that quarter to which gravity inclines. Secondly, all canals reaching from the centre to the surface must balance at the centre, otherwise the preponderating column will subside, and press up the other, and the form of the surface will change. And, lastly, any particle of the whole mass must be *in equilibrio*, being equally pressed in *every* direction. These three conditions seem sufficient for ensuring the equilibrium of the whole.

323. These conditions will be secured in an elliptical fluid spheroid of uniform density turning round its axis, *if the gravity at the pole be to the equatorial gravity, diminished by the centrifugal force arising from the rotation, as the radius of the equator to the semiaxis.*

We shall first demonstrate, that in this case gravity will be every where perpendicular to the spheroidal surface.

Let p express the polar gravity, e the primitive equatorial gravity, and c the centrifugal force at the surface of the equator, and let $e - c, = s$, be the sensible gravity remaining at the equator. Then, by hypothesis, we have $p : s = CE : CA$. Considering the state of any individual particle P on the surface of the spheroid, we perceive, that that part of its compound gravitation which is in a direction perpendicular to the plane of the equator is not affected by the rotation. It still is, therefore, to the force p at the pole as $P\,d$ to AC (318.) But the other constituent of the whole gravitation of P, which is estimated perpendicular to the axis, is diminished by the centrifugal force of rotation, and this diminution is in proportion to its distance from the axis, that is, in proportion to this primitive constituent of its whole gravitation. Therefore, its remaining gravity, in a direction perpendicular to the axis, is still in the proportion of its distance from it. And this is the case with every individual particle. Each particle, therefore, may still be considered as urged only by two forces; one of which is perpendicular to the equator, and proportional to its distance from it; and the other is perpendicular to the axis, and proportional to its distance from it. Therefore, if we draw a line P v u, so that d C may be to $d\,v$ as $p \times$ E C to $s \times$ A C, P v will be the direction of the compound force of gravity at P, as affected by the rotation.

But by hypothesis $p : s =$ E C : A C; therefore $p \times$ E C : $s \times$ A C $=$ E C^2 : A C^2, and E C^2 : A C^2 $= d$ C : $d\,v$, $=$ P u : P v. But, by conic sections, if P u be to P v as E C^2 to A C^2, the line P v u is perpendicular to the tangent to the ellipse in the point P, and therefore to the spheroidal surface, or to the surface of the still ocean.

Thus, then, the first condition is secured, and the superficial waters of the ocean will have no tendency to move in any direction. Having therefore ascertained a suitable *direction* of the affected gravitation of P, we may next inquire into its intensity.

324. The sensible gravity of any superficial particle P is every where to the polar gravity as the line P u (the normal terminating in the axis) to the radius of meridional curvature at the pole; and it is to the sensible gravity at the equator as the portion P v of the same normal terminating in the equator is to the radius of meridional curvature at the equator. For it was shewn (319.) to be to the force at E as P u to E C. If, therefore, the radius of the equator be taken as the measure of the gravitation there, P u will measure the sensible gravitation at P. And since the ultimate situation of the point u, when P is at the pole, is the centre of curvature of the ellipse at A, the radius of curvature there will measure the polar gravity. That is, the sensible gravity at the equator is to the gravity at the pole, as the radius of the equator to the radius of polar curvature. By a perfectly similar process of reasoning, it is proved, that if the gravity at the pole be measured by A C, the gravity at P is measured by P v, and

at the equator by the radius of curvature of the ellipse in E.

325. *Cor.* 1. The sensible gravity in every point P of the surface is reciprocally as the perpendicular $C\,t$ from the centre on the tangent in that point. For every where in the ellipse, $C\,t \times P\,u = C\,E^2$, and $C\,t \times P\,v = C\,A^2$, as is well known.

326. *Cor.* 2. The central gravity of every superficial particle P, that is, its absolute gravity $P\,u$, or $P\,v$ estimated in the direction P C, is inversely proportional to its distance from the centre, that is, the central gravity at P is to the central gravity at E as E C to P C, and to the polar gravity as A C to P C. For, if the gravity $P\,v$ be reduced to the direction P C by drawing $v\,o$ perpendicular to C P, $P\,o$ will measure this central gravity. Now, it is well known that $P\,o \times P\,C$ is every where $= A\,C^2$; and, in like manner, $P\,n \times P\,C = E\,C^2$. Therefore Po, or $P\,n$, are every where reciprocally as P C.

Hence it follows that the sensible increment of gravity in proceeding from the equator to the pole is very nearly as the square of the sine of the latitude; for, without entering on a more curious investigation, it is plain that the increments of gravity, when so minute in comparison with the whole gravity, are very nearly as the decrements of the distance. Now, in a spheroid very little compressed, these decrements are in that proportion. It may be demonstrated that in the latitude where $\sin.^2 = \frac{1}{3}$, namely, lat. 35° 16′, the gravity is the same as to a perfect sphere of the same capacity, having for its radius the semidiameter of the ellipse in that point. It is also a distinguishing property of this latitude that, if this semidiameter be produced, the gravitation of a particle, at any distance in this direction, is the same as to a perfect sphere of the same capacity. This is not the case in any other direction.

327. *Cor.* 3. Lastly, the force estimated in the direction $P\,d$ is to the force in the direction $P\,a$ as $E\,C^2 \times P\,d$ to

$AC^2 \times Pa$. For we had (318.) $f, Pd : f, Pa = A \times EC \times Pd : E \times AC \times Pa$, which, by substituting p and s for A and E, it becomes $p \times EC \times Pd : s \times AC \times Pa, = EC^2 \times Pd : AC^2 \times Pa$.

Hitherto we have considered only the particles on the surface of the spheroid. But we must know the condition of a particle any where within it.

328. A particle p, in any internal point of a diameter, has its sensible gravity in the direction perpendicular to the surface of a concentric and similar spheroid passing through the particle. For the gravity at p is compounded of forces perpendicular to the axis and to the equator, and proportional to the distances from them, and therefore proportional to the similar forces acting on the particle P (312.) Therefore the compound force of p will be parallel, and in the same proportion, to the compound force P v of P, and must therefore be perpendicular to the tangent of the surface in p. It is as $p\,v'$.

329. *Cor.* Hence we must infer that if there were a cavern at p, containing water, the surface of this still water would be a part of the spheroidal surface $a\,e\,b\,q$. Should this cavern extend all the way to e or a, the water should arrange itself according to the surface; or, if $e\,r\,p$ be a pipe or conduit, the water in it should be still, except so far as it is affected by the pressures of the columns A a and P p and E e (these pressures will be proved to be equal.)

It would seem, from these premises, that if the elliptical spheroid consist of different fluids, which do not mix, and which differ in density, they will be disposed in concentric similar elliptical strata, so that their bounding surfaces shall be similar. The proof of this seems the same with what is received for a demonstration of the horizontal surface of the boundary between water and oil contained in a vessel. Accordingly, this has been supposed by many respectable writers, as a thing that needed no other proof. But this is by no means the case. It can be strictly demonstrated that the denser fluids occupy the lowest place, and that the strata become less and less eccentric as we approach the centre, where the ultimate evanescent figure may be denominated a spherical point. It may be seen, even at present, that they cannot be similar, unless homogeneous. For, without this condition, it cannot be generally demonstrated that the gravitation of a particle p to the equator, and to the axis, is as the distance from them, which is the foundation of all the subsequent demonstrations.

330. In the next place, all rectilineal columns, extending from the centre to the surface, will balance in the centre. For, drawing $v\,o$, $v'\,o'$ perpendicular to P C, it is plain that $P\,o$ and $p\,o'$ represent the gravities of P and p estimated in the direction P C. Now $P\,o : p\,o' = P\,C : p\,C$. Therefore the gravitation of the whole column, or the pressure on C, is represented by $\frac{P\,o \times P\,C}{2}$ (320.) Now, in the ellipse $P\,o \times P\,C = C\,A^2$, a constant quantity. Therefore the pressure of every column at C is the same. In like manner, the pressure of the columns $C\,p$ and $C\,a$ are equal, and therefore also the pressures of $P\,p$, $E\,e$, and $A\,a$, at p, e, and a, are all equal.

331. Lastly, any particle of the fluid is equally pressed in every direction, and if the whole were fluid, would be in *equilibrio*, and remain at rest.

To prove this, let $P\,p$ (Fig. 40.) be a column reaching from P to the surface, and taken in any direction, but, first, in one of the meridional planes, of which A B is the axis, and E Q the intersection by the equatorial plane. In the tangent $A\,\alpha$ take $A\,a$ equal to E C, and $A\,\alpha$ equal to A C. Draw $a\,C\,e$ and $\alpha\,C\,\varepsilon$ to the tangent $E\,\varepsilon$ at the equator. It is evident that $E\,e = A\,C$, and $E\,\varepsilon = E\,C$. Through p and P draw the lines $p\,L\,l$, $N\,P\,z$, parallel to E C, and the lines $p\,N\,\varphi$, $I\,P\,\zeta$ parallel to A B. Draw also $I\,K\,k$ parallel to E C.

Since, by hypothesis, the whole forces at A and E are inversely as A C and E C, A *a* and E *e* are as the forces acting at A and E. Consequently, the weights of the columns F D, L Z, and K L, will be represented by the areas F *f d* D, L *l x* Z, and K *k l* L (320.)

All the pressures or forces which act on the particles of the column *p* P may be resolved into forces acting parallel to A C, and forces acting parallel to E C, and the force acting on each particle is as its distance from the axis to to which it is directed (318.) Therefore the whole force with which the column *p* P is pressed in the direction A C is to the force with which the column O P is pressed in the same direction, as the number of particles in *p* P to the number in O P, that is, as *p* P to O P. But there is only a part of this force employed in pressing the particles in the direction of the canal. Another part merely presses the fluid to the side of the canal *p* P. Draw O *g* perpendicular to *p* P. The force acting in the direction A C on any particle in *p* P is to its efficacy in the direction *p* P, as O P to *g* P, that is, as *p* P to O P. Therefore, the pressure which the particle P sustains in the direction *p* P from the action of all the particles in *p* P in the direction A C, is precisely equal to the pressure it sustains from the action of the column O P, acting in the same direction A C. But it has been shewn (320.) that the pressure of O P in the direction A C is precisely the same with the weight of the column L Z, which weight is represented by the area L *l x* Z.

In the very same manner, the whole pressure on P in the direction *p* P arising from the pressure of each of the particles in *p* P in the direction E C, is precisely the same with the pressure on P, arising from the pressure of the column N P in this direction E C, that is, it is equal to the weight of the column F D, which is represented by the area F *f d* D.

Because E ε is equal to E C, we have F φ δ D =

$\frac{CF^2 - CD^2}{2}, = \frac{Lp^2 - LO^2}{2}, = \frac{pO \times Om}{2}$. And in like manner, $K\kappa\lambda L = \frac{IO \times Oi}{2}$. But $pO \times Om : IO \times Oi = EC^2 : AC^2$, and therefore

$$F\varphi\delta : K\kappa\lambda L = EC^2 : AC^2$$

but $K\kappa\lambda L : KklL = AC : EC$

and $FfdD : F\varphi\delta D = AC : EC$, therefore

$$FfdD : KklL = EC^2 \times AC^2 : AC^2 \times EC^2,$$

that is, in the ratio of equality. Now the area $KklL$ represents the weight of the column K L, or the pressure exerted in the direction A C by the column I O.

Thus it appears that when the forces acting on the particles in the column pP are estimated in the direction of the canal, the pressure exerted on the particle P is equal to the united pressures of the columns O P and I O acting in the direction A C, that is, to the pressure of the fluids in the canal I P in its own direction.* Therefore the fluid in the canal I P will balance the fluid in the canal p P, and the particle P will have no tendency to move in either direction. And, since this is equally true, whatever may be the direction of the canal Pp, or Pϖ, it follows that the particle P is equally pressed in every direction in the plane of the figure, and would remain at rest, if the whole spheroid were fluid.

But now let the canal Pp be in a plane different from a meridional plane (as in Fig. 41.) In whatever direction Pp is disposed, a plane may be made to pass through it, perpendicular to the plane E e Q q of the equator of the speroid. Let p I q i e be this plane. Its section with the spheroid will be an ellipse, similar to the generating ellipse A E B Q, as is well known. Let the meridional section A E B Q pass through the point P of the canal p P. It will cut the section e I q i in a line I P i perpendicular to its intersection e q with the equator of the spheroid, and therefore parallel to the axis a c b of the section, if it do not coincide with this axis. Let C D E be the semidiameter of the generating ellipse which passes through the intersection D of I i and e q; and draw P Z parallel to D C, and P z parallel to e q cutting a c b in z, and join z Z and c C. It is plain that the plane passing through the axis A B of the spheroid and the axis a b of the section e I q i is perpendicular to that section (for it besects e q, which is a chord of the equatorial circle L e Q q), and that the planes D c C and P z Z are parallel, and the angles at c and z right angles.

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* The student must not confound this with a composition of two pressures or forces N P and O P, composing a pressure or force p P. There is no such composition in the present case. It is only meant that the pressure in the direction p P arising from the gravitation of the particles in the canal, is the same, in respect of magnitude, with the pressure in the direction I P, arising from the gravitation of the fluid in I P.

Let us now consider the forces which act on the particles of fluid in the canal p P. They are, as before, all resolvable into two, one of them parallel to A C, and the other perpendicular to it. Thus, the particle P is urged by a force in the direction P D parallel to A C, and proportional to its distance P D from the equator of the spheroid. It is also urged by a force in the direction P Z perpendicular to A C, and proportional to its distance P Z. This force P Z, may be resolved into P z and z Z. The force z Z remains the same, for all the particles in the canal p P, z Z being equal to c C. But the force P z is always proportional to the distance of the particle in the canal p P from the axis a c b of the section e I q i. It is also to the axipetal force in the direction P Z as P z to P Z.

Moreover, it has been shewn (327.) that the force in the direction P Z is to the force in the direction P D in the ratio of A $C^2 \times$ P Z to E $C^2 \times$ P D, that is (on account of the similarity of the sections A E B Q and $a\,e\,b\,q$), as $a\,c^2 \times$ P Z to $e c^2 \times$ P D. Therefore the force in the direction P z is to the force in the direction P D as $a\,c^2 \times$ P z to $e\,c^2 \times$ P D. Wherefore, since from these elements it has been proved already that the whole pressure on P in the canal p P, lying in the plane A E B Q, is equal to the pressure of the canal I P, it follows that the pressure of the canal p P, lying in the plane $a\,e\,b\,q$ is also equal to the pressure of the canal I P.

Thus it now appears that the particle P is urged in every direction with the same force by the fluid in any rectilineal canal whatever reaching to the surface. It is therefore *in equilibrio*; and, as it is taken at random, in any part of the spheroid, the whole fluid spheroid is *in equilibrio*.

We also see that the whole force with which any particle P is pressed in any direction whatever is to the pressure at the centre C as the rectangle I P i to A C². For that is the proportion of the pressure of the canal I P to that of the canal A C; and all canals terminating in the centre exert equal pressures.

332. It is now demonstrated that a mass of uniformly dense matter, influenced in every particle by gravitation, and so constituted that an equilibrium of force on every particle is necessary for the maintenance of its form, may exist, with a motion of rotation, in the form of an elliptical spheroid, if there be a proper adjustment between the proportion of the two axes and the time of the rotation. Whatever may be the proportion of the axes of an oblate spheroid, there is a rapidity of rotation which will induce that proportion between the undiminished gravity at the pole and the diminished gravity on the surface of the equator, which is required for the preservation of that form. But it has not been proved that a fluid sphere, when set in motion round its axis, must assume the form of an elliptical spheroid, but only that this is a possible form. This was all that Newton aimed at, and his proof is not free from

reasonable objections. The great mathematicians since the days of Newton have done little more. They have not determined the figure that a fluid sphere, or a nucleus covered with a fluid, *must* assume when set in motion round its axis. But they have added to the number of conditions that must be implemented, in order to produce *another kind* of assurance than an elliptical spheroid will answer the purpose, and by this limitation have greatly increased the difficulty of the question. M. Clairaut, who has carried his scruples farther than the rest, requires, besides the three conditions which have been shewn to consist with the permanence of the elliptical form, that it also be demonstrated, 1*mo*, That a canal of any form whatever must every where be *in equilibrio*: 2*do*, That a canal of any shape, reaching from one part of the surface, through the mass, or along the surface, to any other part, shall exert no force at its extremities: 3*tio*, That a canal of any form, returning into itself, shall be *in equilibrio* through its whole extent.

333. I apprehend that in the case of uniform density, all these conditions are involved in the proposition in art. (331.) For we can suppose the canal p P of Fig. 41. to communicate with the canal P δ. It has been shewn that they are *in equilibrio* in P. The canal 4 β may branch off from P γ. These are *in equilibrio* in the point 4. The canal 3 α may branch off at 3, and they will be still *in equilibrio*; and the canal 2 1 will be *in equilibrio* with all the foregoing. Now these points of derivation may be multiplied, till the polygonal canal p P 4 3 2 1 becomes a canal of continual curvature of any form. In the next place, this canal exerts no force at either end. For the *equilibrium* is proved in every state of the canal p P—it may be as short as we please—it may be evanescent, and actually cease to have any length, without any interruption of the *equilibrium*. Therefore, there is no force exerted at its extremity to disturb the form of the surface. It may be observed that this

very circumstance proves that the direction of gravity is perpendicular to the surface. And it must be observed that the perpendicularity of gravity to the surface is not employed in demonstrating this proposition. The whole rests on the propositions in art. 316, 317, and 318, both of which we owe to Mr M'Laurin.

334. Having now demonstrated the competency of the elliptical spheroid for the rotation of a planet, we proceed to investigate the precise proportion of diameters which is required for any proposed rotation. For example, What protuberancy of the equator will diffuse the ocean of this Earth uniformly, consistently with a rotation in $23^h\ 56'\ 04''$, the planet being uniformly dense?

Let p and e express the primitive gravity of a particle placed at the pole and at the surface of the equator, arising solely from the gravitation to every particle in the spheroid, and let c represent the centrifugal tendency at the surface of the equator, arising from the rotation. We shall have an elliptical spheroid of a permanent form, if AC be to EC as $e-c$ is to p (323.) We must therefore find, first of all, what is the proportion of p to e resulting from any proportion of A C to E C.

To accomplish this in general terms with precision, appeared so difficult a task, even to Newton, that he avoided it, and took an indirect method, which his sagacity shewed him to be perfectly safe; and even this was difficult. It is in the complete solution of this problem that the genius of M'Laurin has shewn itself most remarkable both for acuteness and for geometrical elegance. It is not exceeded (in the opinion of the first mathematicians) by any thing of Archimedes or Apollonius. For this reason, it is to be regretted that we have not room for the series of beautiful propositions that are necessary in his method. We must take a shorter course, limited indeed to spheroids of very small eccentricity (whereas the method of M'Laurin extends to any degree of eccentricity), but with this limitation, perfectly exact, and abundantly easy and simple. It is, in its chief steps, the method followed by M. Boscovich.

335. Let A E B Q (Fig. 42.) represent the terrestrial spheroid, nearly spherical, and let A *e* B *q* and E *a* Q *b* represent the inscribed and circumscribed spheres. With the axis and parameter A B describe the parabola A F G, drawing the ordinates B D F, E C H, &c. Describe also the curve line A I L G, such, that we have, in every point of it, A B : A D = D F : D I; A B : A C = C H : C L, &c.

Our first aim shall be to find an expression and value of the polar gravity. We may conceive the spheroid as a sphere, on which there is spread the redundant matter contained between the spherical and the spheroidal surfaces. We know the gravitation of the polar particle A to the sphere, and now want to have the measure of its gravitation to this redundant matter. Suppose the figure to turn round the axis A B. The semiellipsis A E B will generate a spheroidal surface; the semicircle A *e* B will generate a spherical surface, and the intercepted portions P *p*, E *e*, &c. of the ordinates will generate flat rings of the redundant matter. As the deviation from a sphere is supposed very small (E *e* not exceeding the 500dth part of E Q,) we may suppose, without any sensible error, that A *p* is the distance of A from the whole of the ring generated by P *p*.

Proceeding on this assumption, we say that the gravitation of A to the rings generated by P *p*, E *e*, &c. is proportional to the portions F I, H L, &c. of the coresponding ordinates D F, C H, &c., and that the gravitation of A to the whole redundant matter may be expressed by the surface A F H G L I A comprehended between the lines A F H G and A I L G.

For, the absolute gravitation of A to the ring P *p* is directly as the surface of the ring, and inversely as the square of its distance from A. Now, the surface of the ring is as its breadth, and its circumference jointly. Its breadth P *p*, and also its circumference, being proportional to $D p$, the surface is proportional to $D p^2$. The absolute gravitation is therefore proportional to $\frac{D p_2}{A p^2}$. This may be resolved into forces in the directions $A D$ and $D p$. The force in the direction $D p$ is balanced by an equal force on the other side of the axis. Therefore, to have the gravitation in the direction of the axis, the value of the absolute gravitation in the direction $A p$ must be reduced in the proportion of $A p$ to $A D$. It therefore becomes $\frac{D p^2 \times A D}{A p^2 \times A p}, = \frac{D p^2 \times A D}{A p^3}$, or, which is the same thing, $\frac{D p^2 \times A D \times A p}{A p^4}$. But $A p^2 = A B \times A D$, and $A p^4 = A B^2 \times A D^2$. Also $D p^2 = A D \times D B$. Therefore the value last found becomes $\frac{AD \times DB \times AD \times Ap,}{A B^2 \times A D^2}$ which is equal to, or the same thing with $\frac{D B \times A p.}{A B^2}$ Since $A B^2$ is a constant quantity, the gravitation in the direction $A C$ to the ring generated by $P p$ is proportional to $D B \times A p$.

It is very obvious that $D F$, $C H$, $B G$, &c. are respectively equal to $A p$, $A e$, $A B$, &c. Therefore the gravitation to the matter in the ring generated by $P p$ is proportional to $D B \times D F$.

Now, by the construction of the curve line $A L G$, we have $A B : A D = D F : D I$

therefore $A B : D B = D F : I F$

and $A B \times I F = D B \times DF$

Therefore, since $A B$ is constant, $I F$ is proportional to $D B \times D F$, that is, to the gravitation to the ring generated by $P p$. Therefore the gravitation to the whole redundant matter may be represented by the space $A H G L A$.

Let π be the periphery of a circle of which the radius is 1. The circumference of that generated by $E e$

will be $\pi \times C\,e$, and its surface $= \pi \times C\,e \times E\,e$, and the absolute gravitation to it is $\frac{\pi \times C\,e \times E\,e}{A\,e^2}$, or $\frac{\pi \times C\,e \times E\,e}{2\,A\,C^2}$, that is, $\frac{\pi \times E\,e}{2\,A\,C}$. This, when reduced to the direction A C becomes $\frac{\pi \times E\,e \times A\,C}{2\,A\,e \times A\,C}$, that is, $\frac{\pi \times E\,e}{2\,A\,e}$, or, $\frac{\pi \times E\,e \times A\,e}{2\,A\,e^2}$. And because $A\,e^2 = 2\,A\,C^2$, and $L\,H = \frac{1}{2}\,C\,H, = \frac{1}{2}\,A\,e$, the reduced gravitation becomes $\frac{\pi \times E\,e}{2\,A\,C^2} \times L\,H$.

This being the measure or representative of the gravitation to the material surface or ring generated by E e, the gravitation to the whole redundant matter contained between the spheroid and the inscribed sphere will be represented by $\frac{\pi \times E\,e}{2\,A\,C^2}$ multiplied by the space comprehended between the curve lines A F G and A L G. We must find the value of this space.

The parabolic space A H G B A is known to be $= \frac{2}{3}\,A\,B \times B\,G, = \frac{2}{3}\,A\,B^2$. The square of D I is proportional to the cube of B D. For, by the construction of the curve $A\,B^2 : A\,D^2 = D\,F^2 : D\,I^2$, and $D\,I^2 = \frac{A\,D^2 \times D\,F^2}{A\,B^2}, = \frac{A\,D^2}{A\,B} \times \frac{D\,F^2}{A\,B}, = \frac{A\,D^2}{A\,B}\,A\,D, = \frac{A\,D^3}{A\,B}$. Therefore D I is proportional to $A\,D^{\frac{3}{2}}$, and the area A B G L A is $= \frac{2}{5}\,A\,B \times B\,G, = \frac{2}{5}\,A\,B^2$. Take this from the parabolic area $\frac{2}{3}\,A\,B^2$, and there remains $\frac{4}{15}\,A\,B^2$ for the value of A L G H A. This is equal to $\frac{16}{15}\,A\,C^2$.

Now, the gravitation of A to the redundant matter was shewn to be $= A\,L\,G\,H\,A \times \frac{\pi \times E\,e}{2\,A\,C^2}$. This now becomes $\frac{16}{15}\,A\,C^2 \times \frac{\pi \times E\,e}{2\,A\,C^2}$, or $\frac{8}{15}\,\pi \times E\,e$. Such is the gravitation of a particle in the pole of the spheroid to the redundant matter spread over the inscribed sphere.

The gravitation of a particle situated on the surface of the equator to the same redundant matter is not quite so obvious as the polar gravity, but may be had with the same accuracy, by means of the following considerations.

836. Let A B *a b* (Fig. 48.) represent an oblate spheroid, formed by rotation round the shorter axis B *b* of the generating ellipse, and viewed by an eye situated in the plane of its equator. Let A E *a e* be the circumscribed sphere. This spheroid is deficient from the sphere by two meniscuses or cups, generated by the rotation of the lunulæ A E *a* BA and A *e a b* A.

Now, suppose the same generating ellipse A B *a b* A to turn round its longer axis A *a*. It will generate an oblong spheroid, touching the oblate spheroid in the whole circumference of one elliptical meridian, viz. the meridian A B *a b* A which passes through the poles A and *a* of this oblong spheroid. It touches the equator of the oblate spheroid only in the points A and *a*, and has the diameter A*a* for its axis. This oblong spheroid is otherwise wholly within the oblate spheroid, leaving between their surfaces two meniscuses of an oblong form. This may be better conceived by first supposing, that both the spheroids and also the circumscribed sphere are cut by a plane P G *g p*, perpendicular to the axis A *a* of the oblong spheroid, and to the plane of the equator of the oblate spheroid. Now, suppose that the whole figure makes the quarter of a turn round the axis B *b* of the oblate spheroid, so that the pole *a* of the oblong spheroid comes quite in front, and is at C, the eye of the spectator being in the axis produced. The equator of the oblong spheroid will now appear a circle O B *o' b* O, touching the oblate spheroid in its poles B and *b*. The section of the plane P *p* with the circumscribed sphere will now appear as a circle P′ R *p′ r*. Its section with the oblate spheroid will appear an ellipse R G′ *r g′*

similar to the generating ellipse A B *a b*, as is well known. And its section with the oblong spheroid will now appear a circle I G′ *i g′* parallel to its equator O B *o b*. P *p* is equal to P′ *p′*, and G *g* to G′ *g′*. Thus it appears, that as every section of the oblate spheroid is deficient from the concomitant section of the circumscribed sphere by the want of two lunulæ R P′ *r* G′ and R *p′ r g′*, so it exceeds the concomitant section of the oblong spheroid by two lunulæ G′ R *g′* I and G′ *r g′ i*. It is also plain, that if these spheroids differ very little from perfect spheres, as when E B does not exceed $\frac{1}{100}$ of E *e*, the deficiency of each section G *g* from the concomitant section of the circumscribed sphere is very nearly equal to its excess above the concomitant section of the inscribed oblong spheroid. It may safely be considered as equal to one-half of the space contained between the circles on the diameters P′ *p′* and G′ *g′*,* in the same way that we considered the lunula A P E B *e p* A of Fig. 42. as one-half of the space contained between the semicircles A *e* B and *a* E *b*.

From this view of the figure, it appears, that the gravitation of a particle *a* in the equator of the oblate spheroid to the two cups or meniscuses R P′ *r* G′ and R *p′ r g′*, by which the oblate spheroid is less than the circumscribed sphere, may be computed by the very same method that we employed in the last proposition. But instead of computing (as in last proposition) the gravitation of *a* to the ring generated by the revolution of P G, (Fig. 43.) that is, to the surface contained between the two circles R P′ *r p′* and I G′ *i g′*, we must employ only the two lunulæ R P′ *r* G′ R and R *p′ r g′* R. In this way, we may account the gravitation to the deficient matter (or the deficiency of gravitation) to be one-half of the quantity determined by that proposition, and therefore $= \frac{4}{15}\,\pi \times \mathrm{E}\,e$ of Fig. 42. The last proposition gave us the gravitation to all the matter by which the spheroid exceeded the inscribed sphere. The present proposition gives the gravitation to all the matter by which it falls short of the circumscribed sphere.

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* For the circumscribed circle is to the ellipse as the ellipse to the inscribed circle. When the extremes differ so little, the geometrical and arithmetical mean will differ but insensibly.

337. We can now ascertain the primitive gravitation at the pole and at the equator, by adding or subtracting the quantities now found to or from the gravitation to the spheres. Let r be the radius of the sphere, and πr the circumference of a great circle. The diameter is $2\,r$. The area of a great circle is $\frac{\pi r^2}{2}$, and the whole surface of the sphere is $2\,\pi r^2$, and its solid contents is $\frac{2}{3}\,\pi r^3$. Therefore, since the gravitation to a sphere of uniform density is the same as if all its matter were collected in its centre, and is as the quantity of matter directly, and as the square of the distance r inversely, the gravitation to a sphere will be proportional to $\frac{2}{3}\,\frac{\pi r^3}{r^2}$, that is, to $\frac{2}{3}\,\pi r$.*

Now, let A E B Q (Fig. 42.) be an oblate spheroid, whose poles are A and B. The gravity of a particle A to the sphere whose radius is A C is $\frac{2}{3}\,\pi \times \mathrm{AC}, = \frac{2}{3}\,\pi \times \mathrm{E\,C} - \frac{2}{3}\,\pi \times \mathrm{E}\,e$, or $\frac{2}{3}\,\pi \times \mathrm{E\,C} - \frac{10}{15}\,\pi \times \mathrm{E}\,e$. Add to this its gravitation $\frac{8}{15}\pi \times E\,e$, to the redundant matter. The sum is evidently $\frac{2}{3}\pi \times E\,C - \frac{2}{15}\pi \times E\,e$.

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* I beg leave to mention here a circumstance which should have been taken notice of in art. 319, when the first principles of spherical attractions were established. It was shewn, that the gravitation of the particle P to the sperical surface, generated by the rotation of the arch A D′ T, is equal to its gravitation to the surface generated by the rotation of B D T. Therefore, if P be infinitely near to A, so that the surface generated by A D′ T may be considered as a point or single particle, the gravitation to that particle is equal to the gravitation to all the rest of the surface; that is, it is one-half of the whole gravitation. If we suppose P and A to coincide, that is, make P one of the particles of the surface, its gravitation to the spherical surface will be only one-half of what it was when it was without the surface; and if we suppose P adjoining to A internally, it will exhibit no gravitation at all.

The gravitation of the particle E on the surface of the equator to a sphere whose radius is E C is $\frac{2}{3}\pi \times E\,C$. From this subtract its deficiency of gravitation, viz. $\frac{4}{15}\pi \times E\,e$, and there remains the equatorial primitive gravity $= \frac{2}{3}\pi \times E\,C - \frac{4}{15}\pi \times E\,e$.

Therefore, in this spheroid, the polar gravity is to the equatorial gravity as $\frac{2}{3}\pi \times EC - \frac{2}{15}\pi \times E\,e$ to $\frac{2}{3}\pi \times EC - \frac{4}{15}\pi \times E\,e$, or (dividing all by $\frac{2}{3}\pi$) as $E\,C - \frac{1}{5}E\,e$ to $E\,C - \frac{2}{5}E\,e$, or (because $E\,e$ is supposed to be very small in comparison with E C) as E C to $E\,C - \frac{1}{5}E\,e$. In general terms, let g represent the mean gravity, p the polar, and e the equatorial gravity, r the radius of the inscribed sphere, and x the elevation $E\,e$ of the equator above the inscribed sphere. We have, for a general expression of this proportion of the primitive gravitations, $p : e = r + \frac{1}{5}x : r$, or (because x is very small in comparison with r), $p : e = r : r - \frac{1}{5}x$. This last is generally the most convenient, and it is exact, if r be taken for the equatorial radius.

338. Had the spheroid been prolate (oblong), the same reasoning would have given us $p : e = r : r + \frac{1}{5}x$.

I may add here, that the gravitation at the pole of an oblong spheroid, the gravitation at the equator of an oblate spheroid (having the same axes) and the gravitation to the circumscribed sphere, on any point of its surface, are proportional, respectively, to $\frac{1}{3}r + \frac{1}{15}x$; $\frac{1}{3}r + \frac{1}{5}x$; and $\frac{1}{3}r + \frac{1}{3}x$.*

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* Many questions occur, in which we want the gravitation of a particle P′ situated in the direction of any diameter C P (Fig. 42.) Draw the conjugate diameter C M, and suppose the spheroid cut by a plane passing through C M perpendicular to the plane of the figure. This section will be an ellipse, of which the semiaxes are C M and C E ($= r + x$). A circle, whose radius is the mean proportional

339. It now appears, as was formerly hinted, (322.) that we cannot have an elliptical spheroid of uniform density, in which the gravitation at the pole is to that at the equator as the equatorial radius to the polar radius. This would make $p:e = r:r - x$, a ratio five times greater than that which results from a gravitation proportional to $\frac{1}{d^2}$.

Thus have we obtained, with sufficient accuracy, the ratio of polar and equatorial gravity, unaffected by any external force; and we are now in a condition to tell what velocity of rotation will so diminish the equatorial gravitation, that the remaining gravity there shall be to the polar gravity as A C to E C.

340. Let c be taken to represent the centrifugal tendency generated at the surface of the equator by the rotation of the planet round its axis, and let the other symbols be retained. The sensible gravity at the equator is $e - c$, the polar gravity p, and the excess of the equatorial radius above the semiaxis r is x.

We have shewn, (337.) that the primitive gravities at the pole and the equator are in the ratio of r to $r - \frac{1}{5}x$, or (because x is in a very small part of r) in the ratio of $r + \frac{1}{5}x$ to r. That is, $r : r + \frac{1}{5}x = e : p$. This gives $p = e + \frac{ex}{5r}$. Therefore, the ratio of the *sensible* equato-

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between C M and C E, has the same area with this section, and the gravitation to this circle will be the same (from a particle placed in the axis) with the gravitation to this section. Therefore, as the angle P C M is very nearly a right angle, the gravitation of P′ to the spheroid will be the same with its polar (or axicular) gravitation to another spheroid, whose polar semiaxis is P C, and whose equatorial radius is the mean proportional between C M and C E. This is easily computed. If the arch P E be 35° 16′, a sphere having the radius P C has the same capacity with the spheroid A E B Q (when E e is very small). Hence follows what was said in the note on art. 337.

rial gravity to the gravity at the pole is $e - c : e + \frac{ex}{5r}$, or, very nearly, $e : e + \frac{ex}{5r} + c$. Therefore we must have, for a revolving sphere of small eccentricity,

$$e : e + \frac{ex}{5r} + c = r : r + x$$

and $$e : \frac{ex}{5r} + c = r : x$$

consequently, $$ex = \frac{ex}{5} + rc$$

and $$ex - \frac{ex}{5} \text{ or } \frac{4ex}{5} = rc$$

and $$4ex = 5rc, \text{ and } x = \frac{5rc}{4e}$$

and the ellipticity $\frac{x}{r} = \frac{5c}{4e}$, that is,

Four times the primitive gravity at the equator is to five times the centrifugal force of rotation as the semiaxis to the elevation of the equator above the inscribed sphere.

341. It is a matter of observation, that the diminution of equatorial gravity by the Earth's rotation in $23^h\ 56'\ 4''$ is nearly $\frac{1}{289}$. Therefore, $4 \times 289 : 5 = r : x = 231\frac{1}{5} : 1$, very nearly. This is the ratio deduced by Newton in his indirect and seemingly incurious method. That method has been much criticised by his scholars, as if it could be supposed, that Newton was ignorant that the proportionality employed by him, in a rough way, was not *necessarily* involved in the nature of the thing. But Newton knew that, in the present case, the error, if any, must be altogether insignificant. He did not demonstrate, but assumed as granted, that the form is elliptical, or that an elliptical form is competent to the purpose. His justness of thought has been so repeatedly verified, in many cases as abstruse as this, that it is unreasonable to ascribe it to conjecture, and it should rather, as by Dan. Bernoulli, be

ascribed to his penetration and sagacity. He had so many new wonders to communicate, that he had not time for all the lemmas that were requisite for enabling inferior minds to trace his steps of investigation.

342. When considering the astronomical phenomena, some notice was taken of the attempts which have been made to decide this matter by observation alone, by measuring degrees of the meridian in different latitudes.

But such irregularity is to be seen among the measures of a degree, that the question is still undecided by this method. All that can be made evident by the comparison is, that the Earth is oblate, and much more oblate than the ellipse of Mr Hermann; and that the medium deduction approaches much nearer to the Newtonian form. When we recollect, that the error of one second in the estimation of the latitude induces an error of more than thirty yards in the measure of the degree, and that the form of this globe is to be learned, not from the lengths of the degrees, but from the differences of those lengths, it must be clear, that unless the lengths, and the celestial arc corresponding, can be ascertained with great precision indeed, our inference of the variation of curvature must be very vague and uncertain. The perusal of any page of the daily observations in the observatory of Paris will shew, that errors of 5″ in declination are not uncommon, and errors of 2″ are very frequent indeed. So many circumstances may also affect the measure of the terrestrial arc, that there is too much left to the judgment and *choice* of the observer, in drawing his conclusions. The history of the first measurement of the French meridian by Cassini and La Hire is a proof of this. The degrees seemed to increase to the southward—the observations were affirmed to be excellent—and for some time the Earth was held to be an oblong spheroid. Philosophy prevailed, and this was allowed to be impossible;—yet the observations were still held to be faultless, and the blame was laid on the neglect of circumstances which should have been considered. It was afterwards found, that the deduced measures did not agree with some others of unquestionable authority, but would agree with them if the corrections were left out;—they were left out, and the observations declared excellent, because agreeable to the doctrine of gravitation.*

343. The theory of universal gravitation affords another means of determining the form of the terraqueous globe directly from observation. Mr Stirling says, very justly, that the diminution of gravity, deducible from the remark of M. Richer, and confirmed by many similar observations, gives an incontestable proof, both of the rotation of the Earth, and of its oblate figure. It could not be an oblate figure, and have the ocean uniformly distributed, without turning round its axis; and it could not turn round its axis without inundating the equator, unless it have an oblate form, accompanied with diminished equatorial gravity. By the Newtonian theory, the increments of gravity as we approach the poles are in the duplicate ratio of the sines of the latitude. The increments of the length of a seconds pendulum will have the same proportion. Nothing can be ascertained by observation with greater accuracy than this. For the London artists can make clocks which do not vary one second from mean motion in three or four

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* They were reconciled with the doctrine of gravitation, by attributing the enlargement of the southern degrees to the action of the Pyrenean mountains, and those in the south of France, upon the plummets. But it appears clearly, by the examination of these observations by Professor Celsius, that the observations were very incorrect, and some of them very injudiciously contrived (See Phil. Trans. No 457. and 386.) The palpable inaccuracies gave such latitude for adjustment, that it was easy for the ingenious Mr Mairan to combine them in such a manner as to deduce from them inferences in support of opinions altogether contradictory of those of the academy. Have we not a remarkable example of the doubtfulness of such measures, in the measurement of the Lapland degree? It is found to be almost 200 fathoms too long.

days. We need not measure the change in the length of the pendulum, a very delicate task—but the change of its rate of vibration by a change of place, which is easily done; and we can thus ascertain the force of gravity without an error of one part in 86400. This surpasses all that can be done in the measurement of an angle. Accordingly, the ellipticities deduced from the experiments with pendulums are vastly more consistent with each other, and it were to be wished that these experiments were more repeated. We have but very few of them.

344. Yet even these experiments are not without anomalies. Since, from the nature of the experiment, we cannot ascribe these to errors of observation, and the doctrine of universal gravitation is established on too broad a foundation to be called in question for these anomalies, philosophers think it more reasonable to attribute the anomalies to local irregularity in terrestrial gravity. If, in one place, the pendulum is above a great mass of solid and dense rock, perhaps abounding in metals, and, in another place, has below it a deep ocean, or a deep and extensive stratum of light sand or earth, we should certainly look for a retardation of the pendulum in the latter situation. The French academicians compared the vibrations of the same pendulum on the sea-shore in Peru, and near the top of a very lofty mountain, and they observed, that the retardation of its motion in the latter situation was not so great as the removal from the centre required, according to the Newtonian theory, viz. in the proportion of the distance (the gravity being in the inverse duplicate proportion).*

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* The length of a pendulum vibrating seconds was found to be 439,21 French lines on the sea-shore at Lima; when reduced to time at Quito, 1466 fathoms higher, it was 438,88; and on Pichinka, elevated 2434 fathoms, it was 438,69. Had gravity diminished in the inverse duplicate ratio of the distances, the pendulum at Quito should have been 438,80, and at Pichinka it should have been 438,56.

But it should not be so much retarded. The pendulum was not raised aloft in the air; but was on the top of a great mountain, to which, as well as to the rest of the globe, its gravitation was directed. Some observations were reported to have been made in Switzerland, which shewed a greater gravitation on the summit of a mountain than in the adjacent valleys; and much was built on this by the partizans of vortices. But, after due inquiry, the observations were found to be altogether fictitious. It may just be noticed here, that some of the anomalies in the experiments with pendulums may have proceeded from magnetism. The clocks employed on those occasions probably had gridiron pendulums, having five or seven iron rods, of no inconsiderable weight. We know, for certain, that the lower end of such rods acquires a very distinct magnetism by mere upright position. This may be considerable enough, especially in the circumpolar regions, to affect the vibration, and it is therefore advisable to employ a pendulum having no iron in its composition.

Although the deduction of the form of this globe, from observations on the variations of gravity, is exposed to the same cause of error which affects the position of the plummet, occasioning errors in the measure of a degree, yet the errors in the variations of gravity are incomparably less. What would cause an error of a whole mile in the measure of a degree, will not produce the $\frac{1}{100}$ part of this error in the difference of gravity.

345. These observations naturally lead to other reflections. Newton's determination of the form of the terraqueous globe, is really the form of a homogeneous and fluid or perfectly flexible spheroid. But will this be the form of a globe, constituted as ours in all probability is, of beds or layers of different substances, whose density probably increases as they are farther down?

This is a very pertinent and momentous question. But this outline of mechanical philosophy will not admit of a discussion of the many cases which may reasonably be proposed for solution. All that can, with propriety, be attempted here, is to give a *general* notion of the change of form that will be induced by a varying density. And even in this, our attention must be confined to some simple and probable case. We shall, therefore, suppose the density to increase as we penetrate deeper, and this in such sort, that at any one depth the density is uniform. It is highly improbable that the internal constitution of this globe is altogether irregular.

346. We shall, therefore, suppose a sphere of solid matter, equally dense at equal distances from the centre, and covered with a less dense fluid; and we shall suppose that the whole has a form suitable to the velocity of its rotation. It is this form that we are to find out. With this view, let us suppose that all the matter, by which the solid globe or nucleus is denser than the fluid, is collected in the centre. We have seen that this will make no change in the gravitation of any particle of the incumbent fluid. Thus, we have a solid globe, covered with a fluid of the same density; and, besides the mutual gravitation of the particles of the fluid, we have a force of the same nature acting on every one of them, directed to the central redundant matter. Now, let the globe liquefy or dissolve. This can induce no change of force on any particle of the fluid. Let us then determine the form of the now fluid spheroid, which will maintain itself in rotation. This being determined, let the globe again become solid. The remaining fluid will not change its form, because no change is induced on the force acting on any particle of the fluid. Call this hypothesis A.

347. In order to determine this state of *equilibrium*, or the form which insures it, which is the chief difficulty, let us form another hypothesis B, differing from A only in this circumstance, that the matter collected in the centre, instead of attracting the particles of the incumbent fluid with a force decreasing in the inverse duplicate ratio of

their distances, attracts them with a force increasing in the direct ratio of their distances, keeping the same intensity at the distance of the pole as in hypothesis A. This fictitious hypothesis, similar to Hermann's, is chosen, because a mass so constituted will maintain the form of an accurate elliptical spheroid, by a proper adjustment of the proportion of its axis to the velocity of its rotation. This will easily appear. For we have already seen that the mutual gravitation of the particles of the elliptical fluid spheroid produces, in each particle, a force which may be resolved into two forces, one of them perpendicular to the axis, and proportional to the distance from it, and the other perpendicular to the equator, and proportional to the distance from its plane. There is now by hypothesis B superadded, on each particle, a force proportional to its distance from the centre, and directed to the centre. This may also be resolved into a force perpendicular to the axis, and another perpendicular to the equator, and proportional to the distances from them. Therefore, the whole combined forces acting on each particle may be thus resolved into two forces in those directions and in those proportions. Therefore, a mass so constituted will maintain its elliptical form, provided that the velocity of its rotation be such that the whole forces at the pole and the equator are inversely as the axes of the generating ellipse. We are to ascertain this form, or this required magnitude of the centrifugal force. Having done this, we shall restore to the accumulated central matter its natural gravitation, or its action on the fluid in the inverse duplicate ratio of the distances, and then see what change must be made on the form of the spheroid in order to restore the *equilibrium*.

348. Let B A *b a* (Fig. 44.) be the fictitious elliptical spheroid of hypothesis B. Let B E *b e* be the inscribed sphere. Take E G, perpendicular to C E, to represent the force of gravitation of a particle in E to the central matter, corresponding to the distance C E or C B. Draw

C G. Draw also A I perpendicular to C A, meeting C G in I. Describe the curve G L R, whose ordinates G E, L A, R M, &c. are proportional to $\frac{1}{CE^2}, \frac{1}{CA^2}, \frac{1}{CM^2}$, &c. These ordinates will express the gravitations of the particles E, A. M, &c. to the central matter by hypothesis A.

In hypothesis A, the gravitation of A is represented by A L, but in hypothesis B it is represented by A I. For, in hypothesis B the gravitations to this matter are as the distances. E G is the gravitation of E in both hypotheses. Now, $EG : AL = CA : CE^2$, but $EG : AI = CE : CA$.—In hypothesis A the weight of the column A is represented by the space A L G E, but by A I G E in hypothesis B. If, therefore, the spheroid of hypotheses B was *in equilibrio*, while turning round its axis, the *equilibrium* is destroyed by merely changing the force acting on the column E A. There is a loss of pressure or weight sustained by the column E A. This may be expresed by the space L G I, the difference between the two areas E G I A and E G L A. But the *equilibrium* may be restored by adding a column of fluid A M, whose weight A L R M shall be equal to L G I, which is very nearly $= \frac{LI \times AE}{2}$.

In order to find the height of this column, produce G E on the other side of E, and make E F to E G as the density of the fluid to the density by which the nucleus exceeded it. E F will be to E G as the gravitation of a particle in E to the globe (now of the same density with the fluid) is to its gravitation to the redundant matter collected in the centre. Now, take D E to represent the gravitation of E to the fluid contained in the concentric spheroid E $\beta e \beta$, which is somewhat less than its gravitation to the sphere E B *e b*. Draw C D N. Then A N represents the gravitation of A to the whole fluid spheroid, by sec. 313. In like manner, N I is the united gravitation of A to both the fluid and the central matter, in the same hypothesis.

But in hypothesis A, this gravitation is represented by N L.

Let N O represent the centrifugal force affecting the particle A, taken in due proportion to N A or N L, its whole gravitation in hypothesis A. Draw C K O. D K will be the centrifugal force at E. The space O K G I will express the whole sensible weight of the fluid in A E, according to hypothesis B, and O K G L will express the same, according to hypothesis A. L G I is the difference, to be compensated by means of a due addition A M.

This addition may be defined by the quadrature of the spaces G E A L and G L I. But it will be abundantly exact to suppose that G L R sensibly coincides with a straight line, and then to proceed in this manner. We have, by the nature of the curve G L R,

$$AL : EG = EC^5 : AC^3$$

Also A H, or $EG : AI = EC : AC$

Therefore $AL : AI = EC^6 : AC^3$

Now, when a line changes by a very small quantity, the variation of a line proportional to its cube is thrice as great as that of the line proportional to the root. H I is the quantity proportional to E A the increment of the root E C. I L is proportional to the variation of the cube, and is therefore very nearly equal to thrice H I,

Therefore since $EG : HI = EC : AE$, we may state $EG : LI = EC : 3AE$,

or $3EG : LI = EC : AE$.

Now, Q O L R may be considered as equal to $QR \times AM$, or as equal to $KG \times AM$, and L G I may be considered as equal to $LI \times \frac{1}{2} AE$, and $2KG \times AM = LI \times AE$.

Therefore $2KG : AE = LI : AM$

but $EC : AE = 3EG : LI$

therefore $2KG \times EC : AE^2 = 3EG : AM$.

and $2KG : \dfrac{AE^2}{EC} = 3EG : AM$

and $$2\,KG : 3\,EG = \frac{AE^2}{EC} : AM$$

That is, twice the sensible gravity at the equator is to thrice the gravitation to the central matter as a third proportional to radius and the elevation of the equator is to the addition necessary for producing the *equilibrium* required in hypothesis A.

This addition may be more readily conceived by means of a construction. Make $AE : Ee = 2\,KG : 3\,EG$. Draw ea parallel to EA, and draw Cem, cutting AN in m. Then am is the addition that must be made to the column AC. A similar addition must be made to every diameter CT, making $2\,KG : 3\,EG = \frac{TV^2}{CV} : Tt$, and the whole will be in *equilibrio*.

349. This determination of the ellipticity will equally suit those cases where the fluid is supposed denser than the solid nucleus, or where there is a central hollow. For EG may be taken negatively, as if a quantity of matter were placed in the centre acting with a repelling or centrifugal force on the fluid. This is represented on the other side of the axis Bb. The space gil in this case is negative, and indicates a diminution of the column ae, in order to restore the *equilibrium*.

350. It is evident that the figure resulting from this construction is not an accurate ellipse. For, in the ellipse, Tt would be in a constant ratio to VT, whereas it is as VT^2 by our construction. But it is also evident, that in the cases of small deviation from perfect sphericity, the change of figure from the accurate ellipse of hypothesis B is very small. The greatest deviation happens when Ee is a maximum. It can never be sensibly greater in proportion to AE than $\frac{5}{4}$ of AE is in proportion to EC, unless the centrifugal force FD be very great in comparison of the gravity DE. In the case of the Earth, where EA is nearly $\frac{1}{230}$ of EC, if we suppose the mean density of the Earth

to be five times that of sea water, $a\,m$ will not exceed $\frac{1}{315444}$ of E C, or $\frac{1}{924}$ of A E.

351. We are not to imagine that, since central matter requires an addition A M to the spheroid, a greater density in the interior parts of this globe requires a greater equatorial protuberancy than if all were homogeneous; for it is just the contrary. The spheroid to which the addition must be made is not the figure suited to a homogeneous mass, but a fictitious figure employed as a step to facilitate investigation. We must, therefore, define its ellipticity, that we may know the shape resulting from the final adjustment.

Let f be the density of the fluid, and n the density of the nucleus, and let $n - f$ be $= q$, so that q corresponds with E G of our constuction, and expresses the redundant central matter (or the central deficiency of matter, when the fluid is denser than the nucleus). Let B C or E C E be r, A E be x, and let g be the mean gravity (primitive), and c the centrifugal force at A. Lastly, let π be the circumference when the radius of the circle is 1.

The gravitation of B to the fluid spheroid is $\frac{2}{3}\pi f r$ (337), and its gravitation to the central matter is $\frac{2}{3}\pi q r$. The sum of these, or the whole gravitation of B, is $\frac{2}{3}\pi n r$. This may be taken for the mean gravitation on every point of the spheroidal surface.

But the whole gravitation of B differs considerably from that of A.

1*mo*, C A, or C E, is to $\frac{1}{5}$ A E as the primitive gravity of B to the spheroid is to its excess above the gravitation (primitive) of A to the same, (337.) That is, $r : \frac{1}{5} x = \frac{2}{3}\pi f r : \frac{2}{15}\pi f x$, and $\frac{2}{15}\pi f x$ expresses this excess.

2*do*, In hypothesis B, we have C E to C A as the gravitation of B or E to the central matter is to the gravitation of A to the same. Therefore C E is to E A as the gravitation of E to this matter is to the excess of A's gravitation to the same. This excess of A's gravitation is expressed by $\frac{2}{3}\pi q x$, for $r : x = \frac{2}{3}\pi q r : \frac{2}{3}\pi q x$.

3tio. Without any sensible error, we may state the ratio of g to c as the ratio of the whole gravitation of A to the centrifugal tendency excited in A by the rotation. Therefore $g : c = \frac{2}{3}\pi n r : \frac{2\pi n r c}{3g}$, and this centrifugal tendency of the particle A is $\frac{2\pi n r c}{3g}$. This is what is expressed by N O in our construction.

The whole difference between the gravitations of B and A is therefore $\frac{2}{15}\pi f x - \frac{2}{3}\pi q x + \frac{2\pi n r c}{3g}$. The gravitation of B is to this difference as $\frac{2}{3}\pi n r$ to $\frac{2}{15}\pi f x - \frac{2}{3}\pi q x + \frac{2\pi n r c}{3g}$ or (dividing all by $\frac{2}{3}\pi n$) as r to $\frac{fx}{5n} - \frac{qx}{n} + \frac{cr}{g}$.

Now the equilibrium of rotation requires that the whole polar force be to the sensible gravitation at the equator as the radius of the equator to the semiaxis (324.) Therefore we must make the radius of the equator to its excess above the semiaxis as the polar gravitation to its excess above the sensible equatorial gravitation. That is $r : x = r : \frac{fx}{5n} - \frac{qx}{n} + \frac{cr}{g}$, and therefore $x = \frac{fx}{5n} - \frac{qx}{n} + \frac{cr}{g}$. Hence we have $\frac{cr}{g} = x + \frac{qx}{n} - \frac{fx}{5n}$. But $q = n - f$. Therefore $\frac{cr}{g} = x + \frac{nx}{n} - \frac{fx}{n} - \frac{fx}{5n}, = x + x - \frac{6fx}{5n}, = 2x - \frac{6fx}{5n}, = x \times \left(2 - \frac{6f}{5n}\right)$ Wherefore $x = \frac{cr}{g + \left(2 - \frac{6f}{5n}\right)}, = \frac{5ncr}{g \times \overline{10n - 6f}}$, which is more conveniently expressed in this form $x = \frac{5cr}{2g} \times \frac{n}{5n - 3f}$. The species, or ellipticity of the spheroid is $\frac{x}{r}, = \frac{5c}{2g} \times \frac{n}{5n-3f}$.

Such then is the elliptical spheroid of hypothesis B; and we saw that, in respect of form, it is scarcely distinguishable from the figure which the mass will have when the fictitious force of the central matter gives place to the natural force of the dense spherical nucleus. This is true at least in all the cases where the centrifugal force is very small in comparison with the mean gravitation.

We must therefore take some notice of the influence which the variations of density may have on the form of this spheroid. We may learn this by attending to the formula

$$\frac{x}{r} = \frac{5c}{2g} \times \frac{n}{5n-3f}.$$

The value of this formula depends chiefly on the fraction $\frac{n}{5n-3f}$.

352. If the density of the interior parts be immensely greater than that of the surrounding fluid, the value of this fraction becomes nearly $\frac{1}{5}$, and $\frac{x}{r}$ becomes nearly $= \frac{c}{2g}$, and the ellipse nearly the same with what Hermann assigned to a homogeneous fluid spheroid.

If $n = 5f$; then $\frac{n}{5n-3f} = \frac{5}{22}$; and, in the case of the Earth, $\frac{x}{r}$ would be nearly $= \frac{1}{508,6}$, making an equatorial elevation of nearly 7 miles.

353. If $n = f$, the fraction $\frac{n}{5n-3f}$ becomes $\frac{1}{2}$, and $\frac{x}{r} = \frac{5c}{4g}$, which we have already shewn to be suitable to a homogeneous spheroid, with which this is equivalent. The

protuberance or ellipticity in this case is to that when the nucleus is incomparably denser than the fluid in the proportion of 5 to 2. This is the greatest ellipticity that can obtain when the fluid is not denser than the nucleus.

Between these two extremes, all other values of the formula are competent to homogeneous spheroids of gravitating fluids, covering a spherical nucleus of greater density, either uniformly dense, or consisting of concentric spherical strata, each of which is uniformly dense.

From this view of the extreme cases, we may infer in general, that as the incumbent fluid becomes rarer in proportion to the nucleus, the ellipticity diminishes. M. Bernoulli (Daniel), misled by a gratuitous assumption, says in his theory of the tides that the ellipticity produced in the aëreal fluid which surrounds this globe will be 800 times greater than that of the solid nucleus; but this is a mistake, which a juster assumption of *data* would have prevented. The aëreal spheroid will be sensibly less oblate than the nucleus.

It was said that the value of the formula depended chiefly on the fraction $\frac{n}{5n - 3f}$. But it depends also on the the fraction $\frac{5c}{2g}$, increasing or diminishing as c increases or diminishes, or as g diminishes or increases. It must also be remarked that the theorem $\frac{\varepsilon}{r} = \frac{5c}{4g}$ for a homogeneous spheroid was deduced from the supposition that the eccentricity is very small (See sect. 335, 340.) When the rotation is very rapid, there is another form of an elliptical spheroid, which is in that kind of *equilibrium*, which, if it be disturbed, will not be recovered, but the eccentricity will increase with great rapidity, till the whole dissipates in a round flat sheet. But within this limit there is a kind of stability in the *equilibrium*, by which it is recovered when it is disturbed. If the rotation be too rapid, the spheroid becomes more oblate, and the fluids which accumulate about the equator, having less velocity than that circle, retard the motion. This goes on however some time, till the true shape is overpassed, and then the accumulation relaxes. The motion is now too slow for this accumulation, and the waters flow back again toward the poles. Thus an oscillation is produced by the disturbance, and this is gradually diminished by the mutual adhesion of the waters, and by friction, and things soon terminate in the resumption of the proper form.

354. When the density of the nucleus is less than that of the fluid, the varieties which result in the form from a variation in the density of the fluid are much greater, and more remarkable. Some of them are even paradoxical. Cases, for example, may be put, (when the ratio of n to f differs but very little from that of 3 to 5), where a very small centrifugal force, or very slow rotation, shall produce a very great protuberance, and, on the contrary, a very rapid rotation may consist with an oblong form like an egg. But these are very singular cases, and of little use in the explanation of the phenomena actually exhibited in the solar system. The *equilibrium* which obtains in such cases may be called a *tottering equilibrium*, which, when once disturbed, will not be again recovered, but the dissipation of the fluid will immediately follow with accelerated speed. Some cases will be considered, on another occasion, where there is a deficiency of matter in the centre, or even a hollow.

355. The chief distinction between the cases of a nucleus covered with an equally dense fluid, and a dense nucleus covered with a rarer fluid, consists in the difference between the polar and equatorial gravities; for we see that the difference in shape is inconsiderable. It has been shewn already that, in the homongeneous spheroid of small eccentricity, the excess of the polar gravity above the sensible

equatorial gravity is nearly equal to $\frac{gx}{5r}$ (for $r : \frac{1}{5}x :: g : \frac{gx}{5r}$). When, in addition to this, we take into account the diminution c, produced by rotation, we have $\frac{gx}{5r} + c$ for the whole difference between the polar and the sensible equatorial gravity. But, in a homogeneous spheroid, we have $x = \frac{5cr}{4g}$. Therefore the excess of polar gravity in a homogeneous revolving spheroid is $\frac{c}{4} + c$ or $\frac{5c}{4}$. We may distinguish this excess in the homogeneous spheroid by the symbol E.

356. But, in hypothesis B, the equilibrium of rotation requires that r be to x as g to $\frac{gx}{r}$, and the excess of polar gravity in this hypothesis is $\frac{gx}{r}$. But we have also seen that in this hypothesis, $\frac{x}{r} = \frac{5c}{2g} \times \frac{n}{5n - 3f}$. Therefore the excess of polar gravity in this hypothesis is $\frac{5c}{2} \times \frac{n}{5n - 3f}$. Let this excess be distinguished by the symbol ϵ.

357. The excess of polar gravity must be greater than this in hypothesis A. For, in that hypothesis the equatorial gravity to the fluid part of the spheroid is already smaller. And this smaller gravity is not so much increased by the natural gravitation to the central matter, in the inverse duplicate ratio of the distance, as it was increased by the fictitious gravity to the same matter, in the direct ratio of the distances. The second of the three distinctions noticed in sect. 596. between the gravitations of B and A was $-\frac{qx}{n}$. This must now be changed into $+\frac{2qx}{n}$ as

may easily be deduced from sect. 348, where $-\frac{qx}{n}$ is represented by H I in Fig. 44, and the excess, forming the compensation for hypothesis A is represented by H L, nearly double of H I, and in the opposite direction, diminishing the gravitation of A. The difference of these two states is $\frac{3qx}{n}$, by which the tendency of A to the central matter in hypothesis A falls short of what it was in hypothesis B. Therefore, as $\frac{fx}{5n} - \frac{qx}{n} + \frac{cr}{g}$ is to $\frac{3qx}{n}$, so is the excess ε, to a quantity ι, which must be added to ε, in order to produce the difference of gravities e, conformable to the statement of hypothesis A. Now, in hypothesis B we had $x = \frac{fx}{5n} - \frac{qx}{n} + \frac{cr}{g}$, and we may, without scruple, suppose x the same in hypothesis A. Therefore $\varepsilon : \iota = x : \frac{3qx}{n}, = 1 : \frac{3q}{n}$, and $\iota = \varepsilon \times \frac{3q}{n} = \varepsilon \times \frac{3n-3f}{n}, = \frac{5c}{2} \times \frac{n}{5n-3f} \times \frac{3n-3f}{n}, = \frac{5c}{2} \times \frac{3n-3f}{5n-3f}$. Add to this ε, which is $\frac{5c}{2} \times \frac{n}{5n-3f}$, and we obtain for the excess e of polar gravity in hypothesis A $= \frac{5c}{2} \times \frac{4n-3f}{5n-3f}$

358. Let us now compare this excess of polar gravity above the sensible equatorial gravity in the three hypotheses: 1*st*, A, suited to the fluid surrounding a spherical nucleus of greater density: 2*d*, B, suited to the same fluid, surrounding a central nucleus which attracts with a force proportional to the distance: and, 3*d*, C, suited to a homogeneous fluid spheroid, or enclosing a spherical nucleus of equal density. These excesses are

A $\quad \frac{5c}{2} \times \frac{4n-3f}{5n-3f}$

B $\quad \frac{5c}{2} \times \frac{n}{5n-3f}$

C $\quad \frac{5c}{4}$, or $\frac{5c}{4} \times \frac{5n-3f}{5n-3f}$.

It is evident that the sum of A and B is $\frac{5c}{2} \times \frac{5n-3f}{5n-3f}$, which is double of C, or $\frac{5c}{4} \times \frac{5n-3f}{5n-3f}$, and therefore C is the arithmetical mean between them.

Now we have seen that $\frac{5c}{2g} \times \frac{4n-3f}{5n-3f}$ expresses the ratio of the excess of polar gravity to the mean gravity in the hypothesis A. We have also seen that $\frac{5c}{2g} \times \frac{n}{5n-3f}$ may safely be taken as the value of the ellipticity in the same hypothesis. It is not perfectly exact, but the deviation is altogether insensible in a case like that of the Earth, where the rotation and the eccentricity are so moderate. And, lastly, we have seen that the same fraction that expresses the ratio of the excess of polar gravity to mean gravity, in a homogeneous spheroid, also expresses its ellipticity, and that twice this fraction is equal to the sum of the other two.

359. Hence may be derived a beautiful theorem, first given by M. Clairaut, that *the fraction expressing twice the ellipticity of a homogeneous revolving spheroid is the sum of two fractions, one of which expresses the ratio of the excess of polar gravity to mean gravity, and the other expresses the ellipticity of any spheroid of small eccentricity, which consists of a fluid covering a denser spherical nucleus.*

If, therefore, any other phenomena give us, in the case of a revolving spheroid, the proportion of polar and equatorial gravities, we can find its ellipticity, by subtracting the fraction expressing the ratio of the excess of polar gravity to the mean gravity from twice the ellipticity of a homogeneons spheroid. Thus, in the case of the Earth, twice the ellipticity of the homogeneous spheroid is $\frac{1}{115}$. A medium of seven comparisons of the rate of pendulums gives the proportion of the excess of polar gravity above the mean gravity $= \frac{1}{180}$. If this fraction be subtracted from $\frac{1}{115}$, it leaves $\frac{1}{319}$ for the medium ellipticity of the Earth. Of these seven experiments, five are scarcely different in the result. Of the other two, one gives an ellipticity not exceeding $\frac{1}{335}$. The agreement, in general, is incomparably greater than in the forms deduced from the comparisons of degrees of the meridian. All the comparisons that have been published concur in giving a considerably smaller eccentricity to the terraqueous spheroid than suits a homogeneous mass, and which is usually called Newton's determination. It is indeed his determination, on the supposition of homogeneity; but he expressly says, that a different density in the interior parts will induce a different form, and he points out some supposititious cases, not indeed very probable, where the form will be different. Newton has not conceived this subject with his usual sagacity, and has made some inferences that are certainly inconsistent with his law of gravitation.

That the protuberancy of the terrestrial equator is certainly less than $\frac{1}{231}$ proves the interior parts to be of a greater mean density than the exterior, and even gives us some means for determining how much they exceed in density. For, by making the fraction $\frac{5e}{2g} \times \frac{4n - 3f}{5n - 3f} = \frac{1}{180}$, as indicated by the experiments with pendulums, we can find the value of n.

360. The length of the seconds pendulum is the measure of the accelerating force of gravity. Therefore, let l be this length at the equator, and $l + d$ the length at the pole. We have $\frac{5c}{2g} \times \frac{4n - 3f}{5n - 3f} = \frac{d}{l}$, whence

$\frac{4n-3f}{5n-3f}=\frac{2gd}{5cl}$. This equation, when properly treated, gives $\frac{n}{f}=\frac{15cl-6gd}{20cl-10gd}$, &c. &c.*

The same principles may be applied to any other planet as well as to this Earth. Thus, we can tell what portion of the equatorial gravity of Jupiter is expended in keeping bodies on his surface, by comparing the time of his rotation with the period of one of his satellites. We find that the centrifugal force at his equator is $\frac{8}{93}$ of the whole gravity, and from the equation $\frac{5cr}{4g}=x$, we should infer, that if Jupiter be a homogeneous fluid or flexible spheroid, his equatorial diameter will exceed his polar axis nearly 10 parts in 113, which is not very different from what we observe; so much, however, as to authorise us to conclude, that his density is greater near the centre than on his surface.

These observations must suffice as an account of this subject. Many circumstances of great effect are omitted, that the consideration might be reduced to such simplicity as to be discussed without the aid of the higher geometry. The student who wishes for more complete information must consult the elaborate performances of Euler, Clairaut, D'Alembert, and La Place. The dissertation of Th. Simpson on the same subject is excellent. The dissertation of F. Boscovich will be of great service to those who are less versant in the fluxionary calculus, that author having every where endeavoured to reduce things to a geometrical construction. To these I would add the Cosmographia of Frisius, as a very masterly performance on this part of his subject.

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* We have information very lately of the measurement of a degree, by Major Lambton, in the Mysore in India, with excellent instruments. It lies in lat. 12° 32′, and its length is 60494 British fathoms. We are also informed, by Mr Melanderhielm of the Swedish academy, that the measure of the degree in Lapland by Maupertuis is found to be 208 toises too great. This was suspected.

It were desirable that another element were added to the problem, by supposing the planet to consist of coherent flexible matter. It is apprehended, that this would give it a form more applicable to the actual state of things. If a planet consist of such matter, ductile like melted glass, the shape which rotation, combined with gravitation and this kind of cohesion, would induce, will be considerably different from what we have been considering, and susceptible of great variety, according to the thickness of the shell of which it is supposed to consist. The form of such a shell will have the chief influence on the form which will be assumed by an ocean or atmosphere which may surround it. If the globe of Mars be as eccentric as the late observations indicate it to be, it is very probable that it is hollow, with no great thickness. For the centrifugal force must be exceedingly small.

361. The most singular example of this phenomenon that is exhibited in the solar system, is the vast arch or ring which surrounds the planet Saturn, and turns round its axis with most astonishing rapidity. It is above 200000 miles in diameter, and makes a complete rotation in ten hours and thirty-two minutes. A point on its surface moves at the rate of 1000½ miles in a minute, or nearly 17 miles in one beat of the clock, which is 58 times as swift as the Earth's equator.

M. La Place has made the mechanism of this motion a subject of his examination, and has prosecuted it with great zeal and much ingenuity. He thinks that the permanent state of the ring, in its period of rotation, may be explained, on the supposition that its parts are without connexion, revolving round the planet like so many satellites, so that it may be considered as a vapour. It appears to me, that this is not at all probable. He says, that the observed inequalities in the circle of the ring are necessary for keeping it from coalescing with the planet. Such inequalities seem incompatible with its own constitution, being inconsistent with the *equilibrium* of forces among incoherent bodies. Besides, as he supposes no cohesion in it, any inequalities in the constitution of its different parts cannot influence the general motion of the whole *in the manner he supposes*, but merely by an inequality of gravitation. The effect of this, it is apprehended, would be to destroy the permanency of its construction, without securing, as he imagines, the steadiness of its position. But this seems to be the point which he is eager to establish; and he finds, in the numerous list of possibilities, conditions which bring things within his general equation for the *equilibrium* of revolving spheroids; but the equation is so very general, and the conditions are so many, and so implicated, that there is reason to fear, that, in some circumstances, the *equilibrium* is of that kind that has no stability, but, if disturbed in the smallest degree, is destroyed altogether, being like the *equilibrium* of a needle poised upright on its point. There is a stronger objection to M. La Place's explanation. He is certainly mistaken in thinking, that the period of the rotation of the ring is that which a satellite would have at the same distance. The second Cassinian satellite revolves in $65^h\ 44'$, and its distance is 55,2 (the elongation in seconds). Now $\overline{65^h\ '44'}|^2 : \overline{10^h\ 32\frac{1}{4}}|^2 = 56{,}2^3 : 16{,}4^3$. This is the distance at which a satellite would revolve in $10^h\ 32'$. It must be somewhat less than this, on account of the oblate figure of the planet. Yet even this is less than the radius of the very inmost edge of the ring. The radius of the outer edge is not less than 22¼, and that of its middle is 20.

It is a much more probable supposition (for we can only suppose), that the ring consists of coherent matter. It has been represented as supporting itself like an arch; but this is less admissible than La Place's opinion. The rapidity of rotation is such as would immediately scatter the arch, as water is firted about from a mop. The ring must cohere, and even cohere with considerable force, in order to counteract the centrifugal force, which considerably exceeds its weight. If this be admitted, and surely it is the most obvious and natural opinion, there will be no difficulty arising from the velocity of rotation or the irregularity of its parts. M. La Place might easily please his fancy by contriving a mechanism for its motion. We may suppose that it is a viscid substance like melted glass. If matter of this constitution, covering the equator of a planet, turn round its axis too swiftly, the viscid matter will be thrown off, retaining its velocity of rotation. It will therefore expand into a ring, and will remove from the planet, till the velocity of its equatorial motion correspond with its diameter and its curvature. However small we suppose the cohesive or viscid force, it will cause this ring to stop at a dimension smaller than the orbit of a planet moving with the same velocity.—These seem to be legitimate consequences of what we know of coherent matter, and they greatly resemble what we see in Saturn's ring. This constitution of the ring is also well fitted for admitting those irregularities which are indicated by the spots on the ring, and which M. La Place employs with so much ingenuity for keeping the ring in such a position, that the planet always occupies its centre. This is a very curious circumstance, when considered attentively, and its importance is far from being obvious. The planet and the ring are quite separate. The planet is moving in an orbit round the Sun. The ring accompanies the planet in all the irregularities of its motion, and has it always in the middle. This ingenious mathematician gives strong reasons for thinking, that if the ring were perfectly circular and uniform, although it is *possible* to place Saturn exactly in its centre, yet the smallest disturbance by a satellite or passing comet would be the beginning of a derangement, which would rapidly increase, and, after a very short time, Saturn would be in contact with the inner edge of the ring, never more to separate from it. But if the ring is not uniform, but more massive on one side of the centre than on the other, then the planet and the ring may revolve round a common centre, very near, but not coinciding with the centre of the ring. He also maintains, that the oblate form of the planet is another circumstance absolutely necessary for the stability of the ring. The redundancy of the equator, and flatness of the ring, fit these two bodies for acting on each other like two magnets, so as to adjust each other's motions.

The whole of this analysis of the mechanism of Saturn's ring is of the most intricate kind, and is carried on by the author by calculus alone, so as not to be instructive to any but very learned and expert analysts. Several points of it, however, might have been treated more familiarly. But, after all, it must rest entirely on the truth of the conjectures or assumptions made for procuring the possible application of the fundamental equations.

362. The Moon presents to the reflecting mind a phenomenon that is curious and interesting. She always presents the same face to the Earth, and her appearance just now perfectly corresponds with the oldest accounts we have of the spots on her disk. These, indeed, are not of very ancient date, as they cannot be anterior to the telescope. But this is enough to shew that the Moon turns round her axis in precisely the same time that she revolves round the Earth. Such a precise coincidence is very remarkable, and naturally induces the mind to speculate about the cause of it. Newton ascribed it to an oblong oval figure, more dense, or at least heavier, at one end than at the other. This he thought might operate on the Moon somewhat in the way that gravity operates on a pendulum. He defines this figure in Proposition 38. B. III.; and as the eccentricity, or any deviation of its centre of gravity from

that of its figure, is extremely small, the *vis disponens*, by which one diameter is directed towards the Earth, is also very minute, and its operation must be too slow to keep one face steadily turned to the Earth, in opposition to the momentum of rotation round the axis, seven or eight days being all the time that is allowed for producing this effect. Therefore we observe what is called the *Libration* of the Moon, arising from the uniform rotation of the Moon, combined with her unequable orbital motion. One diameter of the Moon is always turned to the upper focus of her orbit, because her angular motion round that focus is almost perfectly uniform, and therefore corresponds with her uniform rotation. But that diameter which is towards us when the Moon is in her apogee or perigee, deviates from the Earth almost six degrees when she is in quadrature. But although, in the short space of eight days, the pendulous force of the Moon cannot prevent this deviation altogether, it undoubtedly lessens it. It is said to produce another effect. If the original projection of the Moon in the tangent of her orbit did not precisely, but very nearly, correspond with the rotation impressed at the same time, this pendulous tendency would, in the course of many ages, gradually lessen the difference, and at last make the rotation perfectly commensurate with the orbital revolution.

But we apprehend, that this conclusion cannot be admitted. For, in whatever way we suppose this arranging force to operate, if it has been able, in the course of ages, to do away some small primitive difference between the velocity of rotation and the velocity of revolution, it must certainly have been able to annihilate a much smaller difference in the position of the Moon's figure, namely, the obliquity of the axis to the plane of the orbit.* It deviates about 1 or 2 degrees from the perpendicular, and it firmly retains this obliquity of position; and no observation can discover any deviation from perfect parallelism of the axis in all situations. It surely requires much less action of the directing force to produce this change in the position of the axis, than to overcome even a very small difference in angular motion, because this last difference accumulates, and makes a great difference of longitude.

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* The axis round which the rotation of the Moon is performed is inclined to the plane of the ecliptic in an angle of 88½°, and it is in-

These considerations seem to prove, that the constant appearance of one and the same part of the Moon's surface has not been produced by the cause suspected by Newton. The coincidence has more probably been original. We have no reason to doubt, that the same consummate skill that is manifest in every part of the system, in which every thing has an accurate adjustment, *pondere et mensurâ*, also made the primitive revolution rotation of the Moon that which we now behold and admire. The manifest subserviency to great and good purposes, in every thing that we in some measure understand, leaves us no room to imagine that this adjustment of the lunar motions is not equally proper.

368. Philosophers have speculated about the nature of that body of faintly shining matter in which the Sun seems immerged, and is called the *zodiacal light*, because it lies in the zodiac. It is rarely perceptible in this climate, yet may sometimes be seen in a clear night in February and March, appearing in the west, a little to the north of where the Sun set, like a beam of faint yellowish grey light, slanting toward the north, and extending, in a pointed or leaf shape, about eight or ten degrees. The appearance is

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clined to the plane of the lunar orbit 82½. It is always situated in the plane passing through the poles of the ecliptic and of the lunar orbit. It therefore deviates about 1½ from the axis of the ecliptic, and 7 from that of the Moon's orbit. The descending node of the Moon's equator coincides with the ascending node of her orbit.

nearly what would be exhibited by a shining or reflecting atmosphere surrounding the Sun, and extending, in the plane of the ecliptic, at least as far as the orbit of Mercury, but of small thickness, the whole being flat like a cake or disk, whose breadth is at least ten times its thickness in the middle.

This has been the subject of speculation to the mechanical philosophers. It is something connected with the Sun. We have no knowledge of any connecting principle but gravitation. But simple gravitation would gather this atmosphere into a globular shape, whereas it is a very oblate disk or lens. Gravitation, combined with a proper revolution of the particles round the Sun, might throw the vapour into this form; and the object of the speculation is to assign the rotation that is suitable to it. If the zodiacal light be produced by the reflection of an atmosphere that is retained by gravity alone, without any mutual adhesion of its particles, it cannot have the form that we observe. The greatest proportion that the equatorial diameter can have to the polar is that of 3 to 2; for, beyond that, the centrifugal force would more than balance its gravitation, and it would dissipate. A very strong adhesion is necessary for giving so oblate a form as we observe in the zodiacal light. Combined with this, it may indeed expand to any degree, by rapidly whirling about, as we see in the manufacture of crown-glass. But how is this whirling given to the solar atmosphere? It may get it by the mere action of the surface of the Sun, in the manner described by Newton in his account of the production of the Cartesian vortices. The surface drags round what is in contact with it. This stratum acts on the next, and communicates to it part of its own motion. This goes on from stratum to stratum, till the outermost stratum begins to move also. All this while, each interior stratum is circulating more swiftly than the one immediately without it. Therefore they are still acting on one another. It is very evident that a permanent state is not acquired, till all turn round in the same time with the Sun's body. This circumstance limits the possible expansion of an atmosphere that does not cohere. It cannot exceed the orbit of a planet which would revolve round the Sun in that time. But the zodiacal light extends much farther.

The discoveries of Dr Herschel on the surface of the Sun, if confirmed by future observation, render this production of the zodiacal light inconceivable. For motions and changes are observed there, which shew a perfect freedom, not constrained by the adhesion of any superior strata. This would give a constant westerly motion on the surface of the Sun.

The difficulty in accounting for this phenomenon is greatly increased by the fact that when a comet passes through this atmosphere, the tail of the comet is not perceptibly affected by it. The comet of 1743 gave a very good opportunity of observing this. It was not attended to; but the descriptions that are given of the appearances of that comet shew clearly that the tail was (as usual) directed almost straight upward from the Sun, and therefore it mixed with this vapour, or whatever it may be, without any mutual disturbance.

It appears, therefore, on the whole, that we are yet ignorant of the nature and mechanism of the zodiacal light.

364. Before concluding this subject, it is not improper to take some notice of an observation to which great importance has been attached by a certain class of philosophers. We shall find it demonstrated in its proper place, that when the force which impels a firm body forward, acts in a direction which passes through its centre of gravity, it merely impels it forward. The body moves in that direction, and every particle moves alike, so that, during its progress, the body preserves the same attitude (so to speak). Taking any tranverse line of the body for a diameter, we express the circumstance by saying, that this

diameter keeps parallel to itself, that is, all its successive positions are parallel to its first position. But, when the moving force acts in a line which passes on one side of the centre of the body, the body not only advances in the direction of the force, but also changes its attitude, by turning round an axis. This is easily seen and understood in some simple cases. Thus, if a beam of timber, floating on water, be pushed or pulled in the middle, at right angles to its length, it will move in that direction, keeping parallel to its first position. But, if it be pushed or pulled in the same direction, applying the force to a point situated at the third of its length, that end is most affected (as we shall see fully demonstrated) and advances fastest, while the remote end is left a little behind. In this particular case, the *initial* motion of all the parts of the beam is the same as if the remote end were held fast for an instant. If the impulse has been nearer to one end than $\frac{1}{3}$ of the length, the remote end will, *in the first instant*, even move a little backward. We shall be able to state precisely the relation that will be observed between the progressive motion and the rotation, and to say how far the centre of the body will proceed while it makes one turn round the axis. We shall demonstrate that this axis, round which the body turns, always passes through its centre of gravity in a certain determined direction.

It very rarely happens that the direction of the impelling force passes exactly through the centre of a body; and accordingly we very rarely observe a body moving forward in free space without rotation. A stone thrown from the hand never does. A bomb-shell, or a cannon-bullet, has commonly a very rapid motion of rotation, which greatly deranges its intended direction.

The speculative philosophers, who wish to explain all the celestial motions mechanically, think that they explain the rotation of the planets, and all the phenomena depending on it, by saying, that one and the same force produced the revolution round the Sun, and the rotation round the axis; and produced those motions, because the direction of the primitive impulse did not pass precisely through the centre of the planet. They even shew, by calculation, the distance between the centre and the line of direction of the impelling force. Thus, they shew, that the point of impulsion on this Earth is distant from its centre $\frac{1}{167}$ of its diameter.

Having thus accounted, as they imagine, for the Earth's rotation, they say that this rotation causes the Earth to swell out all around the equator, and they assign the precise eccentricity that the spheroid must acquire. They then shew, that the action of the Sun and Moon on this equatorial protuberance deranges the rotation, so that the axis does not remain parallel to itself, and produces the phenomenon called the precession of the equinoxes. And thus all is explained mechanically. And on this explanation a conjecture is founded, which leads to very magnificent conceptions of the visible universe. The Sun turns round an axis. Analogy should lead us to ascribe this to the same cause—to the action of a force whose direction does not pass through his centre. If so, the Sun has also a progressive motion through the boundless space, carrying all the planets and comets along with him, just as we observe Jupiter and Saturn carrying their satellites round their annual orbits.

This is, for the most part, perfectly just. A planet turns round its axis and advances; and, therefore, the force which results from the actual composition *of all the forces* which co-operated in producing both motions, does not pass through the centre of the planet, but precisely at the distance assigned by these gentlemen. But there is nothing of explanation in all this. From the manner in which the remark and its application are made, we are misled in our conception of the fact, and the imagination immediately suggests a *single force*, such as we are accustomed to apply in our operations, acting in one precise line, and, therefore, on one point of the body. It is this simplification of conception alone which gives the remark the appearance of explanation. A mathematician may thus give an explanation of a first-rate ship of war turning to windward, by shewing how a rope may be attached to the ship, and how this rope may be pulled, so as to make her describe the very line she moves in. But the seaman knows that this is no explanation, and that he produced this motion of the ship by various manœuvres of the sails and rudder. The only explanation that could be given, corresponding to the natural suggestion by this remark, would be the shewing some general fact in the system, in which this single force may be found that must thus impel the planets eccentrically, and thus urge them into revolution and rotation at once, as they would be urged by a stroke from some other planet or comet. With respect to this Earth, there is not the least appearance of the effect which must have been produced on it, had it been urged into motion by a single force applied to one point. The force has been applied alike to every particle; there is no appearance of any such general force competent to the production of such motions. Nay, did we clearly perceive the existence of such a force, we should be as far from an explanation as ever. It is not enough that Jupiter receives an impulse which impresses both the progressive and rotative motion. His four satellites must receive, each separately, an impulse of a certain precise intensity, and in a certain precise direction, very different in each, and which cannot be deduced from any thing that we know of matter and motion. No principle of general influence has been contrived by the zealous patrons of this system (for it is a system) that gives the smallest satisfaction even to themselves, and they are obliged to rest satisfied with expressing their hopes that it may yet be accomplished.

But suppose that an expert mechanician should shew how the planets, satellites, and comets, may be so placed that an impulse may at once be given to them all, precisely competent to the production of the very motions that we observe, which motions will now be maintained for ever by the universal operation of gravity, we should certainly admire his sagacity and his knowledge of nature. But we still wonder as much as ever at the nice adjustment of all this to ends which have evidently all the excellence that order and symmetry can give, while many of them are indispensably subservient to purposes which we cannot help thinking good. The suggestion of purpose and final causes is as strong as ever. It is no more eluded than it would be, should any man perfectly explain the making of a watch wheel, by shewing that it was the necessary result of the shape and hardness of the files and drills and chisels employed, and the intensity and direction of the forces by which those tools were moved; and having done all this, should say that he had accounted for the nice and suitable form of the wheel as part of a watch. And, with respect to the subsequent oblate form of the planet set in rotation, the mechanical explanation of this is incompatible with the supposition that the revolution and rotation are the effects of one simple force. The oblate form, if acquired by rotation, requires primitive fluidity, which is incompatible with the operation of one simple force as the primitive mover. There is no proof whatever that this Earth was originally fluid; it is not nearly so oblate as primitive fluidity requires; yet its form is so nicely adjusted to its rotation, that the thin film of water on it is distributed with perfect uniformity. We are obliged to grant that a form has been originally given it suitable to its destination, and we enjoy the advantages of this exquisite adjustment.

I acknowledge that the influence of final causes has been frequently and egregiously misapplied, and that these ignorant and precipitate attempts to explain phenomena, or to account for them, and even sometimes to authenticate them, have certainly obstructed the progress of true science. But what gift of God has not been thus abused? A true philosopher will never be so regardless of logic as to adduce final causes as arguments for the reality of any fact; but neither will he have such a horror at the appearances of wisdom, as to shun looking at them. And we apprehend, that unless some

'*Frigidus obstetirit circum præcordia sanguis,*

it is not in any man's power to hinder himself from perceiving and wondering at them. Surely

'*To look thro' nature up to Nature's God,*'

cannot be an unpleasant task to a heart endowed with an ordinary share of sensibility; and the face of nature, expressing the Supreme Mind which gives animation to its features, is an object more pleasing than the mere workings of blind matter and motion.

But enough of this.—We shall close this subject of planetary figures by slightly noticing, for the present, a consequence of the oblate form perceptible in all the planets which turn round their axes; in the explanation of which the penetration of Newton's intellect is eminently conspicuous.

365. In sec. 339, and several following paragraphs, we explained the effects arising from the inclination of the Moon's orbit round the Earth to the plane of the Earth's orbit round the Sun. We saw, for example, that when the intersection of the two planes is in the line A B (Fig. 35.) of quadrature, the Moon is perpetually drawn out of that plane, and her path is continually bent down toward the ecliptic, during her moving along the semicircle ACB, and she describes another path A *c* *b*, crossing the ecliptic in *b*, nearer to A than B is. In the other half of her orbit, the same deviation is continued, and the Moon again crosses the ecliptic before she come to A, crosses her last path near to *c*, and the ecliptic a third time at *d*, and so

on continually. Hence arises the retrograde motion of the nodes of the lunar orbit. We shewed that this obtains, in a greater or less degree, in every position of the nodes, except when they are in the line of syzigy.

What is true of one Moon would be true of any number: It would be true, were there a complete ring of moons surrounding the Earth, not adhering to one another. We saw that the inclination of the orbit is continually changing, being greatest when the nodes are in the line of the syzigies, and smallest when they are in quadrature. Now, if we apply this to a ring of moons, we shall find that it will never be a ring that is all in one plane, except when the nodes are in the syzigies, and at all other times will be warped or out of shape. Now, let the moons all cohere, and the ring becomes stiff; and let this happen when its nodes are in syzigy. It will turn round without disturbance of this sort. But this position of the nodes of the ring soon changes, by the Sun's change of relative situation, and now all the derangements begin again. The ring can no longer go out of shape or warp, because we may suppose it inflexible. But, as in the course of any one revolution of the Moon round the Earth, the inclination of the orbit would either be increased, on the whole, or diminished, on the whole, and the nodes would, on the whole, recede, this effect must be observed in the ring. When the nodes are so situated, that, in the course of one revolution of a single Moon, the inclination will be more increased in one part than it is diminished in another, the opposite actions on the different parts of a coherent and inflexible ring will destroy each other, as far as they are equal, and the excess only will be perceived on the whole ring. Hence, we can infer, with great confidence, that from the time that the nodes of the ring are in syzigy to the time they are in quadrature, the inclination of the ring of moons will be continually diminishing; will be least of all when the Sun is in quadrature with the line of the nodes; and will increase to a maximum, when the Sun again gets into the line of the nodes, that is, when the nodes are in the line of the syzigies. But the inertia of the ring will cause it to continue any motion that is accumulated in it till it be destroyed by contrary forces. Hence, the times of the maximum and minimum of inclination will be considerably different from what is now stated. This will be attended to by and by.

For the same reason, the nodes of the ring will continually recede; and this retrograde motion will be most remarkable when the nodes are in quadrature, or the Sun in quadrature with the line of the nodes; and will gradually become less remarkable, as the nodes approach the line of the syzigies, where the retrograde motion will the least possible, or rather ceases altogether.

All these things may be distinctly perceived, by steadily considering the manner of acting of the disturbing force. This steady contemplation, however, is necessary, as some of the effects are very unexpected.

Suppose now that this ring contracts in its dimensions. The disturbing force, and all its effects, must diminish in the same proportion as the diameter of the ring diminishes. But they will continue the same in kind as before. The inclination will increase till the Sun comes into the line of the nodes, and diminish till he gets into quadrature with them. Suppose the ring to contract till almost in contact with the Earth's surface. The recess of the nodes, instead of being almost three degrees in a month, will now be only three minutes, and the change of inclination in three months will now be only about five seconds.

Suppose the ring to contract still more, and to cohere with the Earth. This will make a great change. The tendency of the ring to change its inclination, and to change its intersection with the ecliptic, still continues. But it cannot now produce the effect, without dragging with it the whole mass of the Earth. But the Earth is at perfect liberty in empty space, and being retained by nothing, yields to every impulse, and therefore yields to this action of the ring.

Now, there is such a ring surrounding the Earth, having precisely this tendency. The Earth may be considered as a sphere, on which there is spread a quantity of redundant matter which makes it spheroidal. The gravitation of this redundant matter to the Sun sustains all those disturbing forces which act on the inflexible ring of moons; and it will be proved, in its proper place, that the effect in changing the position of the globe is $\frac{1}{5}$ of what it would be, if all this redundant matter were accumulated on the equator. It will also appear, that the force by which every particle of it is urged to or from the plane of the ecliptic, is as its distance from that plane. Indeed, this appears already, because all the disturbing forces acting on the particles of this ring are similar, both in direction and proportion, to those which we shewed to influence the Moon in the similar situations of her monthly course round the Earth. Similar effects will therefore be produced.

Let us now see what those effects will be.—The lunar nodes continually recede; so will the nodes of this equatorial ring, that is, so will the nodes of the equator, or its intersection with the ecliptic. But the intersections of the equator with the ecliptic are what we call the Equinoctial Points. The plane of the Earth's equator, being produced to the starry heavens, intersects that seemingly concave sphere in a great circle, which may be traced out among the stars, and marked on a celestial globe. Did the Earth's equator always keep the same position, this circle of the heavens would always pass through the same stars, and cut the ecliptic in the same two opposite points. When the Sun comes to one of those points, the Earth turning round under him, every point of its equator has him in the zenith in succession; and all the inhabitants of the Earth see him rise and set due east and west, and have the day and night of the same length. But in the course of a year, the action of the Sun on the protuberance of our equator deranges it from its former position, in such a manner that each of its intersections with the ecliptic is a little to the westward of its former place in the ecliptic, so that the Sun comes to the intersection about 20′ before he reaches the intersection of the preceding year. This anticipation of the equal division of day and night is therefore called the PRECESSION OF THE EQUINOXES.

The axis of diurnal revolution is perpendicular to the plane of the equator, and must therefore change its position also. If the inclination of the equator to the ecliptic were always the same (23½ degrees), the pole of the diurnal revolution of the heavens (that is, the point of the heavens in which the Earth's axis would meet the concave) would keep at the same distance of 23½ degrees from the pole of the ecliptic, and would therefore always be found in the circumference of a circle, of which the pole of the ecliptic is the centre. The meridian which passes through the poles of the ecliptic and equator must always be perpendicular to the meridian which passes through the equinoctial points, and therefore, as these shift to the westward, the pole of the equator must also shift to the westward, on the circumference of the circle above-mentioned.

But we have seen that the ring of redundant matter does not preserve the same inclination to the ecliptic. It is most inclined to it when the Sun is in the nodes, and smallest when he is in quadrature with respect to them. Therefore the obliquity of the equator and ecliptic should be greatest on the days of the equinoxes, and smallest when the Sun is in the solstitial points. The Earth's axis should twice in the year incline downward toward the ecliptic, and twice in the intervals, should raise itself up again to its greatest elevation.

Something greatly resembling this series of motions may be observed in a child's humming top, when set a spinning on its pivot. An equatorial circle may be drawn on this top, and a circular hole, a little bigger than the top, may be cut in a bit of stiff paper. When the top is spinning very steadily, let the paper be held so that half of the top is above it, the equator almost touching the sides of the hole. When the whirling motion abates, the top begins to stagger a little. Its equator no longer coincides with the rim of the hole in the paper, but intersects it in two opposite points. These intersections will be observed to shift round the whole circumference of the hole, as the axis of the top veers round. The axis becomes continually more oblique, without any periods of recovering its former position, and, in this respect only the phenomena differ from those of the precession.

It was affirmed that the obliquity of the equator is greatest at the equinoxes, and smallest at the solstices. This would be the case, did the redundant ring instantly attain the position which makes an *equilibrium* of action. But this cannot be; chiefly for this reason, that it must drag along with it the whole inscribed sphere. During the motion from the equinox to the next solstice, the Earth's equator has been urged toward the ecliptic, and it must approach it with an accelerated motion. Suppose, at the instant of the solstice, all action of the Sun to cease; this motion of the terrestrial globe would not cease, but would go on for ever, equably. But the Sun's action continuing, and now tending to raise the equator again from the ecliptic, it checks the contrary motion of the globe, and, at length, annihilates it altogether; and then the effect of the elevating force begins to appear, and the equator rises again from the ecliptic. When the Sun is in the equinox, the elevation of the equator should be greatest; but as it arrived at this position with an accelerated motion, it continues to rise (with a retarded motion) till the continuance of the Sun's depressing force puts an end to this rising; and now the effect of the depressing force begins to appear. For these reasons, it happens that the greatest obliquity of the equator to the ecliptic is not on the days of the equinoxes, but about six weeks after, viz. about the first of May and November; and the smallest obliquity is not at midsummer and midwinter, but about the beginning of February and of August.

And thus, we find that the same principle of universal gravitation, which produces the elliptical motion of the planets, the inequalities of their satellites, and determines the shape of such as turn round their axis, also explains this most remarkable motion, which had baffled all the attempts of philosophers to account for—a motion which seemed to the ancients to affect the whole host of heaven; and when Copernicus shewed that it was only an appearance in the heavens, and proceeded from a real small motion of the Earth's axis, it gave him more trouble to conceive this motion with distinctness, than all the others. All these things—*obvia conspicimus nubem pellenti mathesi.*

366. Such is the method which Sir Isaac Newton, the sagacious discoverer of this mechanism, has taken to give us a notion of it. Nothing can be more clear and familiar in general. He has even subjected his explanation to the severe task of calculation. The forces are known, both in quantity and direction. Therefore the effects must be such as legitimately flow from those forces. When we consider what a minute portion of the globe is acted upon, and how much inert matter is to be moved by the force which affects so small a portion, we must expect very feeble effects. All the change that the action of the Sun produces on the inclination of the equator amounts only to the fraction of a second, and is therefore quite insensible. The change in the position of the equinoxes is more conspicuous, because it accumulates, amounting to about 9″ annually, by Newton's calculation. We shall take notice of this calculation at another time, and at present shall only observe that this motion of the equinox is but a small part of the precession actually observed. This is about 50⅓″ annually. It would

therefore seem that the theory and observation do not agree, and that the precession of the equinoxes is by no means explained by it.

367. It must be remarked that we have only given an account of the effect resulting from the unequal gravitation of the terrestrial matter to the Sun. But it gravitates also to the Moon. Moreover, the inequality of this gravitation (on which inequality the disturbance depends) is vastly greater. The Moon being almost 400 times nearer than the Sun, the gravitation to a pound of lunar matter is almost 640,000,000 times greater than to as much solar matter. When the calculation is made from proper data, (in which Newton was considerably mistaken) the effect of the lunar action must very considerably exceed that of the Sun. He was mistaken, in respect to the quantity of matter in the Sun and in the Moon. The transit of Venus, and the observations which have been made on the tides, have brought us much nearer the truth in both these respects. When the calculation is made on such assumptions of the matter in the Sun and Moon as are best supported by observation, we find that the annual precession occasioned by the Sun's action on the equatorial protuberance is about 14″ or 15″, and that produced by the Moon is about 35″. The precession really observed is about 50″, and the agreement is abundantly exact. It must be farther remarked, that this agreement is no longer inferred from a due proportioning of the whole observed precession between the Sun and the Moon, as we were formerly obliged to do; but each share is an independent thing, calculated without any reference to the whole precession. It is thus only that the phenomenon may be affirmed to be truly explained.

368. For this demonstration we are indebted to Dr Bradley. His discovery of what is now called the NUTATION of the Earth's axis, gave us a precise measure of the lunar action which removed every doubt. It therefore must be considered here.

The action of the luminaries on the Earth's equator, by which the position of it is deranged, depends on the magnitude of the angle which the equator makes with the line joining the Earth with the disturbing body. The Sun is never more than 23½ degrees from the equator. But when the Moon's ascending node is in the vernal equinox, she may deviate nearly 29 degrees from it. And when the node is in the autumnal equinox, she cannot go more than 17 degrees from it. Thus, the action of the sun is, from year to year, the same. But as, in 19 years, the Moon's nodes take all situations, the action of the Moon is very variable. It was one of the effects of this variation that Bradley discovered. While the Earth's equator continued to open farther and farther from the line joining the Earth with the Moon, the axis of the Earth was gradually depressed towards the ecliptic, and the diminution of its inclination at last amounted to 18 seconds. Dr Bradley saw this by its increasing the declination of a star properly situated. After nine years, when the Moon was in such a situation that she never went more than 17° from the Earth's equator, the same star had 18″ less declination.

369. This change in the inclination of the Earth's equator is accompanied with a change in the precession of the equinoxes. This must increase as the equator is more open when viewed from the Moon. In the year in which the lunar ascending node is in the vicinity of the vernal equinox, the precession is more than 58″; and it is but 43″ when the node is near the autumnal equinox. These are very conspicuous changes, and of easy observation, although long unnoticed, while blended with other anomalies equally unknown.

Few discoveries in astronomy have been of more service to the science than this of the nutation, and that of aberration, both by Dr Bradley. For till they were known, there was an anomaly, which might sometimes amount to 58″ (the sum of nutation and aberration), and affected every motion and every observation. No theory of any planet could be freed from this uncertainty. But now, we can give to every phenomenon its own proper motions, with all the accuracy that modern instruments can attain. Without these two discoveries, we could not have brought the solution of the great nautical problem of the longitude to any degree of perfection, because we could not render either the solar or lunar tables perfect. The changes in the position of the Earth's axis by nutation, and the concomitant equation of the precession, by recurring in the most regular manner, have given us the most exact measure of the changes in the Moon's action; and therefore gave an incontrovertible measure of her whole action, because the proportion between the variation and the whole action was distinctly known.

This not only completes the practical solution of the problem, but gives the most unquestionable proof of the soundness of the theory, shewing that the oblate form of the Earth is the cause of this nutation of its axis, and establishing the universal and mutual attraction of all matter. It shews with what confidence we may proceed, in following this law of gravitation into all its consequences, and that we may predict, without any chance of mistake, what will be the effect of any combination of circumstances that can be mentioned. And it surely shews, in the most conspicuous manner, the penetration and sagacity of Newton, who gave encouragement to a surmise so singular and so unlike all the usual questions of progressive motion, even in all their varieties. Yet this most recondite and delicate speculation was one of his early thoughts, and is one of the twelve propositions which he read to the Royal Society.

370. It must be acknowledged, however, that this manner of exhibiting the theory of the precession of the equinoxes is not complete, or even accurate in the selection of the physical circumstances on which the proof proceeds. It is merely a popular way of leading the mind to the view of

actions, which are indeed of the same kind with those actually concurring in the production of the effect. But it is not a narration of the real actions. Nor are the effects of those that are employed estimated according to their real manner of acting. The whole is rather a shrewd guess, in which Newton's great penetration enabled him to catch at a very remote analogy between the libration of the Moon and the wavering motion of the Earth's axis. We are not in a condition, in this part of the course, to treat this question in the proper manner. We must first understand the properties of the lever as a mechanical power, and the operation of the connecting forces of firm or rigid bodies. What we have said will suffice however for giving a distinct enough conception of the general effects of the action of remote bodies on a spheroidal planet turning round its axis.* It is scarcely necessary to add that the other planets cannot sensibly influence the motion of the Earth's axis. Their accumulated action may add about $\frac{1}{5}$ of a second to the annual precession of the equinoxes.

The planets Mars, Jupiter, and Saturn, being vastly more oblate than the Earth, must be more exposed to this derangement of the rotative motion. Jupiter and Saturn, having so many satellites, which take various positions round the planet, the problem becomes immensely complicated. But the small inclination of the equator, and the great mass of the planet, and its very rapid rotation, must greatly diminish the effect we are now considering. Mars, being small, turning slowly, and yet being very oblate, must sustain a greater degree of this derangement; and if Mars had a satellite, we might expect such a change in the position of his axis as should become very sensible, even at this distance.

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* To those who wish to study this very curious and difficult problem, I should recommend the solution given by Frisius in the second part of his Cosmographia, as the most perspicuous of any that I am acquainted with. The elaborate performance of Mr Walmesley, Euler, D'Alembert, and La Grange, are accessible only to expert analysts. The essay by T. Simpson, in the Philosophical Transactions, vol. 50, p. 416, is remarkable for its simplicity, but, by employing the symbolical or algebraic analysis, the student is not so much aided by the constant accompaniment of physical ideas, as in the geometrical method of Frisius.

The ring of Saturn must be subject to similar disturbances, and must have a retrogradation of its intersection with the plane of the orbit. Had we nothing to consider but the ring itself, it would be a very easy problem to determine the motion of its nodes. But the proximity, and the oblate form, of the planet, and, above all, the complicated action of the satellites, make it next to unmanageable. It has not been attempted, that I know of. It may (I think) be deduced, from the Greenwich observations since 1750, that the nodes retreat on the orbit of Saturn about 34′ or 36′ in a century, and that their longitude in 1801 was 5ˢ 17° 13′ and 11ˢ 17° 13′. This may be received as more exact than the determination given in art. 142.

I said, in art. 132, that we have seen too little of the motions of Ceres and Pallas to announce the elements of their theories with any thing like precision. But, that they may not be altogether omitted, the following may be received as of most authority:

	Ceres.	Pallas.	Juno.	Vesta.
Mean distance,	2767231	2767123	2.660	2.3632
Eccentri. to m. d. 1.	0.0785	0.2447	0.254	0.1838
Lon. perih. in 1818,	4.227.18	4.1.8—	$1^s 23° 32′ 56″$	$8^s 10° 19′ 36″$
Period (sider.), days,	1682.25	$1703^d 16^h$	1580	1161
Mean lon. Jan. 1818,	$10^s 26° 51′$	$9^s 29° 53′$	$3^s 27° 45′$*	$6^s 24° 40′ 45″$
Inclin. orbit,	10° 38′	24° 37′	13° 3′ 37″	7° 7′ 51″
Lon. node in 1818,	$2^s 20° 45′$	5.22.27	$5^s 21° 6′ 50″$	$3^s 13° 10′ 41″$

These bodies present some very singular circumstances to our study; their distances and periods being almost the same, and their longitudes at present differing very little. They differ considerably in eccentricity, the place of the node, and the inclination of their orbits. They must be greatly disturbed by each other, and by Jupiter.

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* In 1819.

With these observations I might conclude the discussion of the mechanism of the solar system. The facts observed in the appearances of the comets are too few to authorise me to add any thing to what has been already said concerning them. I refer to Newton's Principia for an account of that great philosopher's conjectures concerning the luminous train which generally attends them, acknowledging that I do not think these conjectures well supported by the established laws of motion. Dr Winthorp has given, in the 57th volume of the *Phil. Trans.* a geometrical explanation of the mechanism of this phenomenon that is ingenious and elegant, but founded on a hypothesis which I think inadmissible.

371. No notice has yet been taken of the relations of the solar system to the rest of the visible host of heaven; and we have, hitherto, only considered the starry heavens as affording us a number of fixed points, by which we may estimate the motions of the bodies which compose our system. It will not, therefore, be unacceptable should I now lay before the reader some reflections, which naturally arise in the mind of any person who has been much occupied in the preceding researches and speculations, and which lead the thoughts into a scene of contemplation far exceeding in magnificence any thing yet laid before the reader. As they are of a miscellaneous nature, and not susceptible of much arrangement, I shall not pretend to mark them by any distinctions, but shall take them as they naturally offer themselves.

The fitness for almost eternal duration, so conspicuous in the constitution of the solar system, cannot but suggest the highest ideas of the intelligence of the Great Artist. No doubt these conceptions will be very obscure, and very inadequate; but we shall find, that the farther we advance in our knowledge of the phenomena, we shall see the more to admire, and the more numerous displays of great wisdom, power, and kind intentions.

It is not therefore fearful superstition, but the cheerful anticipation of a good heart, which will make a student of nature even endeavour to form to himself still higher notions of the attributes of the Divine Mind. He cannot do this in a direct manner. All he can do is to abstract all notions of imperfection, whether in power, skill, or benevolent intentions, and he will suppose the Author of the universe to be infinitely powerful, wise, and good.

It is impossible to stop the flights of a speculative mind, warmed by such pleasing notions. Such a mind will form to itself notions of what is most excellent in the designs which a perfect being may form, and it finds itself under a sort of necessity of believing that the Divine Mind will really form such designs. This romantic wandering has given rise to many strange theological opinions. Not doubting (at least in the moment of enthusiasm) that we can judge of what is most excellent, we take it for granted that this creature of our heated imagination must also appear most excellent to the Supreme Mind. From this principle, theologians have ventured to lay down the laws by which God himself must regulate his actions. No wonder that, on so fanciful a foundation as our capacity to judge of what is most excellent, have been erected the most extravagant fabrics; and that, in the exuberance of religious zeal, the Author of all has been described as the most limited Agent in the universe, forced, in every action, to regulate himself by our poor and imperfect notions of what is excellent. We, who vanish from the sight, at the distance of a neighbouring hill—whose greatest works are invisible from the Moon—whose whole habitation is not visible to a spectator in Saturn—shall such creatures pretend to judge of what is supremely excellent?

Let us not pretend even to guess at the specific laws by which the conduct of the Divinity must be directed, except in so far as it has pleased him to declare them to us. We shall pursue the only safe road in this speculation, if we endeavour to discover the laws by which his visible and comprehensible works are actually conducted. The more we discover of these, the more do we find to fill us with admiration and astonishment. The only speculations in which we can indulge, without the continual danger of going astray, are those which enlarge our notions of the scene on which it has pleased the Almighty to display his perfections. This will be the undoubted effect of enlarging the field of our own observation. After examining this lower world, and observing the nice and infinitely various adjustments of means to ends here below, we may extend our observation beyond this globe. Then shall we find that, as far as our knowledge can carry us, there is the same art, and the same production of good effects by beautifully contrived means. We have lately discovered a new planet, far removed beyond the formerly imagined bounds of the planetary world. This discovery shews us, that if there are thousands more, they may be for ever hid from our eyes by their immense distance. Yet *there* we find the same care taken that their condition shall be permanent. They are influenced by a force directed to the Sun, and inversely as the square of the distance from him; and they describe ellipses. This planet is also accompanied by satellites, doubtless rendering to the primary and its inhabitants services similar to what this Earth receives from the Moon. All the comets of whose motions we have any precise knowledge, are equally secured; none seems to describe a parabola or hyperbola, so as to quit the Sun for ever.

This mark of an intention that this noble fabric shall continue for ever to declare itself the work of an Almighty and Kind Hand, naturally carries forward the mind into that unbounded space, of which our solar system occupies so inconsiderable a portion. The mind revolts at the thought that this is studded with stars for no other purpose than to assist the astronomer in his computations, and to furnish a gay spectacle to the unthinking multitude. We see nothing here below, or in our system, which answers but one solitary purpose; and we require that a positive reason shall be given for limiting the Host of Heaven to so ignoble an office. As such has not been given, we indulge ourselves in the pleasing thought that the stars make a part of the universe, no less important in purpose than great in extent. We are justifiable, by what we in some measure understand in supposing each star a sun, the centre of a planetary system, full of enjoyment like our own, and so constructed as to last for ever.

When the philosopher indulges himself in those amazing, but pleasing thoughts, he must regulate his speculations by analogies and resemblances to things more familiarly known to him. We must suppose those systems to resemble our own, and that they are kept together by a gravitation in the inverse duplicate ratio of the distances. For we know that this alone will insure permanency and good order.

But in so doing, we extend the influence of gravity to distances inconceivably greater than any that we have yet considered; and we come at last to believe that gravitation is the bond of connection which unites the most distant bodies of the visible universe, rendering the whole one great machine, for ever operating the most magnificent purposes, worthy of its All-perfect Creator. And, when we see that such a connexion is necessary for this end, we are apt to imagine that gravity is *essential* to or indispensable in that matter that is to be moulded into a world.

But let not our ignorance mislead us, nor let us measure every thing by that small scale which God has enabled us to use, unless we can see some circumstances of resemblance in the appearances, which may justify the application.

[*] A frame of material nature of any kind cannot be conceived by the mind, without supposing that the matter of which it consists is influenced by some active powers, constituting the relations between its different parts. Were there only the mere inert materials of a world, it would hardly be better than a chaos, although moulded into symmetrical forms, unless the spirit of its author were to animate those dead masses, so as to bring forth change, and order, and beauty. Our illustrious Newton therefore says, with great propriety, that the business of a true philosophy is to investigate those active powers, by which the course of natural events, to a very great extent at least, is perpetually governed. Philosophising with this view, he discovered the law of universal gravitation, and has thus given the brightest specimen of the powers of human understanding.

The notion of something like gravity seems inseparable from our conception of any established order of things; for unless some principle of general union obtain among the parts of matter, we can have no conception of the very first formation of the individuals of which a world may be composed.

But *general* gravitation, or that power by which the *distant* bodies belonging to any system are connected, and act on one another, does not seem so indispensably necessary to the very being of the system, as *particular* gravity is to the being of any individual in it. We cannot discern any absurdity in the supposition of bodies, such as the planets, so situated with respect to another great body, such as the Sun, as to receive from it suitable degrees of light and heat, without their having any tendency to approach the Sun, or each other. But then, how far such limitation of gravity may be a possible thing, or how far its indefinite extension in every direction may be involved in its very nature, we cannot tell, until we are able to consider gravity as an effect, and to deduce the laws of its operation from our knowledge of its cause.

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[*] For many of the thoughts in what follows, the reader is indebted to a very ingenious pamphlet, published by Cadell and Davies in 1777, entitled, *Thoughts on General Gravitation.* It is much to be regretted that the author has not availed himself of the successful researches of astronomers since that time, and prosecuted his excellent hints. If it be the performance of the person whom I suppose to be the author, I have such an opinion of his acuteness, and of his justness of thought, that I take this opportunity of requesting him to turn his attention afresh to the subject. His advantages, from his present situation and connexions, are precious, and should not be lost.

That the influence of gravity extends into the boundless void, to the greatest assignable distance, seems to be almost the hinge of the Newtonian philosophy. At least there is nothing that warrants any limit to its action. Father Boscovich, indeed, shews that all the phenomena may be what they are, without this as a necessary consequence. But he is plainly induced to bring forward the limitation in order to avoid what has been thought a necessary consequence of the indefinite extension of gravity; and what he offers is a mere possibility.

Now, if such extension of gravitation be inseparable, in fact, from its nature, then, if all the bodies of our system are at rest in absolute space, no sooner does the influence of general gravitation go abroad into the system, than all the planets and comets must begin to approach the Sun; and in a very small number of days, the whole of the solar system must fall into the Sun, and be destroyed.

But, that this fair order may be preserved, and accommodated to this extended influence of gravity, which appears so essential to the constitution of the several parts of the system, we see a most simple and effectual prevention, by the introduction of *projectile forces* and *progressive motion.* For upon these being now combined, and properly adjusted with the variation of gravity, the planets are made to revolve round the Sun in stated courses, by which their continual approach to the Sun and to one another is prevented, and the adjustment is made with such exquisite propriety, that the perfect order of things is almost unchangeable. This adjustment is no less manifest in the subordinate systems of a primary planet and its satellites, which are not only regulated by their own orbital motions, but are the constant attendants of their primaries in their revolution round the Sun.

In this view of the subject, forasmuch as gravity seems essential to the constitution of all the great bodies of the system, and in so far as its indefinite extension may be inseparable from its nature, it appears that *periodical motion* must be necessary for the permanency and order of every system of worlds whatever.

But here a thought is suggested, which obviously leads to a new and a very grand conception of the universe. If periodical motion be thus necessary for the preservation of a small assemblage of bodies, and if Newton's law present to us the whole host of heaven as one great assemblage affected by gravitation, we must still have recourse to periodical motion, in order to secure the establishment of this grand universal system. For if there be no bounds to the influence of gravitation, and if all the stars be so many suns, the centres of as many systems (as is most reasonable to believe) the immensity of their distance cannot satisfy us for their being able to remain in any settled order. Those that are situated towards the confines of this magnificent creation must forsake their stations, and, with an approach, continually accelerated, must move onwards to the centre of general gravitation, and, after a series of ages, the whole glory of nature must end in a universal wreck.

As the system of Jupiter and his satellites is but an epitome of the great solar system to which he belongs, may not this, in its turn, be a faint representation of that grand system of the universe, round whose centre this Sun, with his attending planets, and an inconceivable multitude of like systems, do in reality revolve according to the law of gravitation? Now, will our anticipation of disorder and ruin be changed into the contemplation of a countless number of nicely-adjusted motions, all proclaiming the sustaining hand of God.

This is indeed a grand, and almost overpowering thought; yet justified both by reason and analogy. The grandeur, however, of this universal system only opens upon us by degrees. If it resemble our solar system in construction, what an inconceivable display of creation is suggested, when we turn our thoughts towards that place which the motions of so many revolving systems are made to respect! Here may be an unthought-of universe of itself, an example of material creation, which must individually exceed all the other parts, though added into one amount. As our Sun is almost four thousand times bigger than all his attendants put together, it is not unreasonable to suppose the same thing here. It is not necessary that this central body should be visible. The great use of it is not to illuminate, but to govern the motions of all the rest. We know, however, that the existence of such a central body is not necessary. Two bodies, although not very unequal, may be projected with such velocities, and in such directions, that they will revolve for ever round their common centre of position and gravitation. But such a system could hardly maintain any regularity of motion when a third body is added. It may indeed be said, that the same transcendent wisdom, which has so exquisitely adapted all the circumstances of our system, may so adjust the motions of an immense number of bodies, that their disturbing actions shall accurately compensate each other. But still the beautiful simplicity that is manifest in what we see and understand, seems to warrant a like simplicity in this great system, and therefore renders the existence of such a great central Regulator of the movements of all the most probable supposition.

Sober reason will not be disposed to revolt at so glorious an extension of the works of God, however much it may overpower our feeble conceptions. Nay, this analogy acquires additional weight and authority even from the transcendent nature of the universe to which it directs our thoughts. Nothing less magnificent seems suitable to a Being of infinite perfections.

But we are not left to mere conjecture in support of this conception of a great universe, connected by mutual powers. There are circumstances of analogy which tend greatly to persuade us of the reality of our conjecture—circumstances which seem to indicate a connexion among the most distant objects of the creation visible from our habitation. The light by which the fixed stars are seen is the same with that by which we behold our Sun and his attending planets. It moves with the same velocity, as we discover by comparing the aberration of the fixed stars with the eclipses of Jupiter's satellites. It is refracted and reflected according to the same laws. It consists of the same colours. No opinion can be formed, therefore, of the solar light, which must not also be adopted with respect to the light of the fixed stars. The medium of vision must be acted on in the same manner by both, whether we suppose it the undulation of an ether, or the emission of matter from the luminous body. In either case, a mechanical connexion obtains between those bodies, however distant, and our system. Such a connexion in mechanical properties induces us to suppose, that gravitation, which we know reaches to a distance which exceeds all our distinct conceptions, extends also to the fixed stars.

If this be really the case, motion must ensue, even in producing the final ruin of the visible universe; and periodic motion is indispensably necessary for its permanency.

If all the fixed stars, and our Sun, were equal, and placed at equal distances, in the angles of regular solids, their mutual ruinous approach could hardly be perceived.

For, in every moment, they would still have the same relative positions, and an increase of brightness is all that could ensue after many ages. But if they were irregularly placed, and unequal, their relative positions would change, with an accelerated motion, and this change might become sensible after a long course of ages. If they have periodical motions, suited to the permanency of the grand system of the universe, the changes of place may be much more sensible; and if we suppose that their difference in brilliancy is owing to the differences in their distance from us, we may expect that these changes will be most sensible in the brightest stars.

Facts are not wanting to prove that such changes really obtain in the relative positions of the fixed stars. This was first observed by that great astronomer, mathematician, and philosopher, Dr Halley. He found, after comparing the observations of Aristillus, Timochares, and Ptolemy, with those of our days, that several of the brighter stars had changed their situation remarkably (See Phil. Trans. No 355.) Aldebaran has moved to the south about 35′. Syrius has moved south about 42′, and Arcturus, also to the south, about 33′. The eastern shoulder of Orion has moved northward about 61′. Observations in modern times shew, that Arcturus has moved in 78 years about 3′ 3″. This is a very sensible quantity, and is easily observed, by means of the small star *b* in its immediate neighbourhood. (See Phil. Trans. LXIII. also 1748; and Mem. Par. 1755.) Syrius, in like manner, increases its latitude about 2′ in a century. (Mem. Par. 1758.) Aldebaran moves very irregularly. The bright star in *Aquila* has changed its latitude 36′ since the time of Ptolemy, and 3′ since the time of Tycho. This is easily seen by its continual separation from the small star δ.

These motions seem to indicate a motion in our system. Most of the stars have moved toward the south. The stars in the northern quarters seem to widen their relative positions, while those in the south seem to contract their distances. Dr Herschel thinks, that a comparison of all these changes indicates a motion of our Sun with his attending planets toward the constellation Hercules. (Phil. Trans. 1788.) A learned and ingenious friend thinks it not impossible to discover this motion by means of the aberration of the stars. Suppose the Sun and planets to be moving toward the Pole-star, and that his motion is 100 times greater than that of the Earth in her orbit (a very moderate supposition, when we compare the orbital motion of the Earth with that of the Moon), every equatorial star will appear about 34′ north of its true place, when viewed through a common telescope, but only 23′ when viewed through a telescope filled with water. The declination of every such star will be 11′ less through a water telescope than through a common telescope. Stars out of the equator will have their declination diminished by a water telescope 11′ × cos. declination.

In 1761, the ingenious Mr Lambert published his Letters on Cosmology (in the German language), in which he has considered this subject with much attention and ingenuity. He treats of the motion of the Sun round a central body—of systems of systems, or milky-ways, carried round an immense body—of systems of such galaxies—and of the great central body of the universe. In these speculations he infers much from final causes, and is often ingeniously romantic. But Lambert was also a true inductive philosopher, and makes no assertion with confidence that is not supported by good analogies. The rotation of the Sun is a strong ground of belief to Mr Lambert that he has also a progressive motion.

Tobias Mayer of Gottingen speaks in the same manner, in some of his dissertations published after his death by Lichtenberg. See also *Bailly's Account of Modern Astronomy*, vol. II. 664, 689. Mayer of Manheim has also published thoughts to this effect. See *Comment. Acad.*

Palatin. IV. Prevost, *Mem. Berlin* 1781. Mitchel, *Phil. Trans.* LVII. 252.

The gravitation to the fixed stars can produce no sensible disturbances of the motions of our system. This gravitation must be inconceivably minute, by reason of the immense distance; and, as they are in all quarters of the heavens, they will nearly compensate each other's action; and the extent of our system being but as a point in comparison with the distance of the nearest star, the gravitation to that star in all the parts of our system must be so nearly equal and parallel, that no sensible derangement can be effected, even after ages of ages.

As a further circumstance of analogy with a periodical motion in the whole visible universe, we may adduce the remarkable periodical changes of brilliancy that are observed in many of the fixed stars.

This was first observed (I think) in a star of the constellation Hydra. Montanari had observed it in 1670, and left some account of it in his papers, which Maraldi took notice of. Maraldi, after long searching in vain, found it in 1704, and saw several alternations of its brightness and dimness, but without being able to ascertain their period. It was long lost again, till Mr Edward Pigot found it in 1786. He determined its period to be 404 days. Since that time, this gentleman, and his father, with a Mr Goodricke, have given more attention to this department of astronomy, and their example has been followed by other astronomers. Mr Pigot has given us, in *Phil. Trans.* 1786, a list of a great number of stars (above fifty) in which such periodical changes have been observed, and has given particular determinations of twelve or thirteen, ascertaining their periods with precision. The whole is followed by some very curious reflections.

Of these stars, one of the most remarkable is χ Cygni, having a period of 415½ days. See *Phil. Trans.* No 343; also *Mem. Acad Paris*, 1719, 1759.

Another remarkable star is ο Ceti, having a period of 334 days. (See *Phil. Trans.* No 134, 346; *Mem. Par.* 1719.)

There is another such, close to γ Cygni.

The double star ζ Lyræ exhibits very singular appearances, the southernmost sometimes appearing double, and sometimes accompanied by more little stars. Grischoff of Berlin is positive that it has planets moving round it.

Some of those stars have very short periods. The most remarkable is Algol, in the head of Medusa. Its period is $2^d\ 20^h\ 49'$, in which its changes are very irregular, although perfectly alike in every period. Its ordinary appearance is that of a star of the second magnitude. It suffers, for about 3½ hours, a reduction to the appearance of a star of the fourth or fifth magnitude.

Mr Goodricke observed similar variations in the star δ Cephei. During $5^d\ 8^h\ 37'$ it is a star of the fifth magnitude. For $1^d\ 13^h$ it is of the second or third. It diminishes during $1^d\ 18^h$; remains 36 hours in its faintest state, and regains its brilliancy in 13 hours more. (*Phil. Trans.* 1786.)

Mr Pigot observed the star η Antinoi to maintain its utmost brilliancy during 44 hours, and then gradually to fade during 62 hours, and, after remaining 30 hours of the fifth magnitude, it regains its greatest brilliancy in 36 hours. (*Phil. Trans.* 1786.)

Whatever may be the cause of these alternations, they are surely very analogous to what we observe in our system, the individuals of which, by varying their positions, and turning their different sides toward us, exhibit alternations of a similar kind; as, for example, the apparition and disparition of Saturn's ring. These circumstances, therefore, encourage us to suppose a similarity of constitution in our system to the rest of the heavenly host, and render it more probable that all are connected by one general bond, and are regulated by similar laws. Nothing is so likely for constituting this connexion as gravitation, and its combination with projectile force and periodic motion tends to secure the permanency of the whole.

But I must at the same time observe, that such appearances in the heavens make it evident, that, notwithstanding the wise provision made for maintaining that order and utility which we behold in our system, the day may come "when the heavens shall pass away like a scroll that "is folded up, when the stars in heaven shall fail, and the "Sun shall cease to give his light." The sustaining hand of God is still necessary, and the present order and harmony which he has enabled us to understand and to admire, is wholly dependent on his will, and its duration is one of the unsearchable measures of his providence. What is become of that dazzling star, surpassing Venus in brightness, which shone out all at once in November 1572, and determined Tycho Brahé to become an astronomer? He did not see it at half an hour past five, as he was crossing some fields in going to his laboratory. But, returning about ten, he came to a crowd of country folks who were staring at something behind him. Looking round, he saw this wonderful object. It was so bright that his staff had a shadow. It was of a dazzling white, with a little of a bluish tinge. In this state it continued about three weeks, and then became yellowish and less brilliant. Its brilliancy diminished fast after this, and it became more ruddy, like glowing embers. Gradually fading, it was wholly invisible after fifteen months.

A similar phenomenon is said to have caused Hipparchus to devote himself to astronomy, and to his vast project of a catalogue of the stars, that posterity might know whether any changes happened in the heavens. And, in 1604, another such phenomenon, though much less remarkable, engaged for some time the attention of astronomers. Nor are these all the examples of the perishable nature of the heavenly bodies. Several stars in the catalogues of Hipparchus, of Ulugh Beigh, of Tycho Brahé, and even of Flamstead, are no more to be seen. They are gone, and have left no trace.

Should we now turn our eyes to objects that are nearer us, we shall see the same marks of change. When the Moon is viewed through a good telescope, magnifying about 150 times, we see her whole surface occupied by volcanic craters; some of them of prodigious magnitude. Some of them give the most unquestionable marks of several successive eruptions, each destroying in part the crater of a former eruption. The precipitous and craggy appearance of the brims of those craters is precisely such as would be produced by the ejection of rocky matter. In short, it is impossible, after such a view of the Moon, to doubt of her being greatly changed from her primitive state.

Even the Sun himself, the source of light, and heat, and life, to the whole system, is not free from such changes.

If we now look round us, and examine with judicious attention our own habitation, we see the most incontrovertible marks of great and general changes over the whole face of the Earth. Besides the slow degradation by the action of the winds and rains, by which the soil is gradually washed away from the high lands, and carried by the rivers into the bed of the ocean, leaving the Alpine summits stripped to the very bone, we cannot see the face of any rock or crag, or any deep gully, which does not point out much more remarkable changes. These are not confined to such as are plainly owing to the horrid operations of volcanoes, but are universal. Except a few mountains, where we cannot confidently say that they are factitious, and which for no better reason we call primitive, there is nothing to be seen but ruins and convulsions. What is now an elevated mountain has most evidently been at the bottom of the sea, and, previous to its being there, has been habitable surface.

It is very true, that all our knowledge on this subject is merely superficial. The highest mountains, and deepest excavations, do not bear so great a proportion to the globe as the thickness of paper that covers a terrestrial globe bears to the bulk of that philosophical toy. We have no authority from any thing that we have seen, for forming any judgment concerning the internal constitution of the Earth. But we see enough to convince us, that it bears no marks of eternal duration, or of existing as it is, by its own energy. No! all is perishable—all requires the sustaining hand of God, and is subject to the unsearchable designs of its Author and Preserver.

There is yet another class of objects in the heavens, of which I have taken no notice. They are called NEBULÆ, or NEBULOUS STARS. They have not the sparkling brilliancy that distinguishes the stars, and they are of a sensible diameter, and a determinate shape. Many of them, when viewed through telescopes, are clusters of stars, which the naked eye cannot distinguish. The most remarkable of these is in the constellation Cancer, and is known by the name *Præsepe*. Ptolemy mentions it, and another in the right eye of Sagittarius. Another may be seen in the head of Orion. Many small clusters have been discovered by the help of glasses. The whole galaxy is nothing else.

But there is another kind, in which the finest telescopes have discovered no clustering stars. Most of them have a star in or near the middle, surrounded with a pale light, which is brightest in the middle, and grows more faint toward the circumference. This circumference is distinct, or well defined, and is not always round. One or two nebulæ have the form of a luminous disk, with a hole in the middle like a millstone. They are of various colours, white, yellow, rose-coloured, &c. Dr Herschel, in several of the late volumes of the Philosophical Transactions, has given us the places of a vast number of nebulæ, with curious descriptions of their peculiar appearances, and a series of most ingenious and interesting reflections on their nature and constitution. His *Thoughts on the Structure of the Heavens* are full of most curious speculation, and should be read by every philosopher.

When we reflect, that these singular objects are not, like the fixed stars, brilliant points, which become smaller when seen through finer telescopes, but have a sensible and measurable diameter, sometimes exceeding 2′; and when we also recollect, that a ball of 200,000,000 miles in diameter, which would fill the whole orbit of the Earth round the Sun, would not subtend an angle of two seconds when taken to the nearest fixed star, what must we think of these nebulæ? One of them is certainly some thousands of times bigger than the Earth's orbit. Although our finest telescopes cannot separate it into stars, it is still probable that it is a cluster. It is not unreasonable to think, with Dr Herschel, that this object, which requires a telescope to find it out, will appear, to a spectator in its centre, much the same as the visible heavens do to us; and that this starry heaven, which to us appears so magnificent, is but a nebulous star to a spectator placed in that nebula.

The human mind is almost overpowered by such a thought. When the soul is filled with such conceptions of the extent of created nature, we can scarcely avoid exclaiming, "Lord, what then is man that thou art mindful "of *him!*" Under such impressions, David shrunk into nothing, and feared that he should be forgotten amongst so many great objects of the divine attention. His comfort and ground of relief from this dejecting thought are remarkable: "But," says he, "thou hast made man but "a little lower than the angels, and hast crowned him "with glory and honour." David corrected himself by calling to mind how high he stood in the scale of God's works. He recognised his own divine original, and his alliance to the Author of all. Now, cheered and delighted, he cries out, "Lord, how glorious is thy name!"

There remains yet another phenomenon, which is very evidently connected with the mechanism of the solar system, and is in itself both curious and important. I mean the tides of our ocean. Although it appears improper to call this an astronomical phenomenon, yet, as it is most evidently connected with the position of the Sun and Moon, we must attribute this connexion, in fact, to a natural connexion in the way of cause and effect.

Of the Tides.

872. It is a very remarkable operation of nature that we observe on the shores of the ocean, when, in the calmest weather, and most serene sky, the vast body of waters that bathe our coasts advances on our shores, inundating all the flat sands, rising to a considerable height, and then as gradually retiring again to the bed of the ocean; and all this without the appearance of any cause to impel the waters to our shores, and again to draw them off. Twice every day is this repeated. In many places, this motion of the waters is tremendous, the sea advancing, even in the calmest weather, with a high surge, rolling along the flats with resistless violence, and rising to the height of many fathoms. In the bay of Fundy, it comes on with a prodigious noise, in one vast wave, that is seen thirty miles off; and the waters rise 100 and 120 feet in the harbour of Annapolis-Royal. At the mouth of the Severn, the flood also comes up in one head, about ten feet high, bringing certain destruction to any small craft that has been unfortunately left by the ebbing waters on the flats; and as it passes the mouth of the Avon, it sends up

that small river a vast body of water, rising forty or fifty feet at Bristol.

Such an appearance forcibly calls the attention of thinking men, and excites the greatest curiosity to discover the cause. Accordingly, it has been the object of research to all who would be thought philosophers. We find very little, however, on the subject, in the writings of the Greeks. The Greeks, indeed, had no opportunity of knowing much about the ebbing and flowing of the sea, as this phenomenon is scarcely perceptible on the shores of the Mediterranean and its adjoining seas. The Persian expedition of Alexander gave them the only opportunity they ever had, and his army was astonished at finding the ships left on the dry flats when the sea retired. Yet Alexander's preceptor, Aristotle, the prince of Greek philosophers, shews little curiosity about the tides, and is contented with barely mentioning them, and saying, that the tides are most remarkable in great seas.

373. When we search after the cause of any recurring event, we naturally look about for recurring concomitant circumstances; and when we find any that generally accompany it, we cannot help inferring some connexion. All nations seem to have remarked, that the flood-tide always comes on our coasts as the Moon moves across the heavens, and comes to its greatest height when the Moon is in one particular position, generally in the south-west. They have also remarked, that the tides are most remarkable about the time of new Moon, and become more moderate by degrees every day, as the Moon draws near the quadrature, after which they gradually increase till about the time of full Moon, when they are nearly of their greatest height. They now lessen every day as they did before, and are lowest about the last quadrature, after which they increase daily, and, at the next new Moon, are a third time at the highest.

These circumstances of concomitancy have been noticed by all nations, even the most uncultivated; and all seem to have concurred in ascribing the ebbing and flowing of the sea to the Moon, as the efficient cause, or, at least, as the occasion, of this phenomenon, although without any comprehension, and often without any thought, in what manner, or by what powers of nature, this or that position of the Moon should be accompanied by the tide of flood or of ebb.

Although this accompaniment has been every where remarked, it is liable to so many and so great irregularities, by winds, by freshes, by the change of seasons, and other causes, that hardly any two succeeding tides are observed to correspond with a precise position of the Moon. The only way, therefore, to acquire a knowledge of the connexion that may be useful, either to the philosopher or to the citizen, is to multiply observations to such a number, that every source of irregularity may have its period of operation, and be discovered by the return of the period. The inhabitants of the sea-coasts, and particularly the fishermen, were most anxiously interested in this research.

374. Accordingly, it was not long after the conquests of the Romans had given them possession of the coasts of the ocean, before they learned the chief circumstances or laws according to which the phenomena of the tides proceed. Pliny says, that they had their source in the Sun and the Moon. It had been inferred, from the gradual change of tides between new Moon and the quadrature, that the Sun was not unconcerned in the operation. Pytheas, a Greek merchant, and no mean philosopher, resident at Marseilles, the oldest Grecian colony, had often been in Britain, at the tin-mines in Cornwall and its adjacent islands. He had observed the phenomena with great sagacity, and had collected the observations of the natives. Plutarch and Pliny mention these observations of Pytheas, some of them very delicate, and, the whole taken together, containing almost all that was known of the subject, till the dis-

coveries of Sir Isaac Newton taught the philosophers what to look for in their inquiries into the nature of the tides, and how to class the phenomena. Pytheas had not only observed, that the tides gradually abated from the times of new and full moon to the times of the quadratures, and then increased again; but, had also remarked, that this vulgar observation was not exact, but that the greatest tide was always two days after new or full Moon, and the smallest was as long after the quadratures. He also corrected the common observation of the tides falling later every day, by observing, that this retardation of the tides was much greater when the moon was in quadrature than when new or full. The tide-day, about the time of new and full Moon, is really shorter by 50′ than at the time of her quadrature.

375. This variation, in the interval of the tides, is called the PRIMING or the LAGGING of the tides, according as we refer them to lunar or solar time. Pytheas probably learned much of this nicety of observation from the Cornish fishermen. By Ælian's accounts, they had nets extended along shore for several miles, and were therefore much interested in this matter.

376. Many observations on the series of phenomena, which completes a period of the tides, are to be found in the books of hydrography, and the instructions for mariners, to whom the exact knowledge of the course of the tides is of the utmost importance. But we never had any good collection of observations, from which the laws of their progress could be learned, till the Academy of Paris procured an order from government to the officers at the ports of Brest and Rochefort, to keep a register of all the phenomena, and report it to the Academy. A register of observations was accordingly continued for six years, without interruption, at both ports, and the observations were published, forming the most complete series that is to be met with in any department of science, astronomy alone

excepted. The younger Cassini undertook the examination of these registers, in order to deduce from them the general laws of the tides. This task he executed with considerable success; and the general rules which he has given contain a much better arrangement of all the phenomena, their periods and changes, than any thing that had yet appeared. Indeed there had scarcely any thing been added to the vague experience of illiterate pilots and fishermen, except two dissertations by Wallis and Flamstead, published in the Philosophical Transactions.

377. It is not likely, notwithstanding this excellent collection of observations, that our knowledge would have proceeded much farther, had not Newton demonstrated that a series of phenomena perfectly resembling the tides resulted from the mutual attraction of all matter. These consequences pointed out to those interested in the knowledge of the tides what vicissitudes or changes to look for —what to look for as the natural or regular series—what they are to consider as mere anomalies—what periods to expect in the different variations—and whether there are not periods which comprehend the more obvious periods of the tides, distinguishing one period from another. As soon as this clue was obtained, every thing was laid open, and without it, the labyrinth was almost inextricable; for in the variations of the tides there are periods in which the changes are very considerable; and these periods continually cross each other, so that a tide which should be great, considered as a certain tide of one period, should be small, considered as a certain tide of another period. When it arrives, it is neither a great nor a small tide, but it prevents both periods from offering themselves to the mere observer. The tides afford a very strong example of the great importance of a theory for directing even our observations. Aided by the Newtonian theory, we have discovered many periods, in which the tides suffer gradual changes, both in their hour and in their height, which com-

monly are so implicated with one another, that they never would have been discovered without this monitor, whereas now, we can predict them all.

378. The phenomena of the tides are, in general, the following:

1. The waters of the ocean rise, from a medium height to that of high water, and again ebb away from the shores, falling nearly as much below that medium state, and then rise again in a succeeding tide of flood, and again make high water. The interval between two succeeding high waters is about $12^h\ 25'$, the half of the time of the Moon's daily circuit round the Earth, so that we have two tides of flood and two ebb tides in every $24^h\ 50'$. This is the shortest period of phenomena observed in the tides. The gradual subsidence of the waters is such that the diminutions of the height are nearly as the squares of the times from high water. The same may be said of the subsequent rise of the waters in the next flood. The time of low water is nearly half way between the two hours of high water; not indeed exactly, it being observed at Brest and Rochefort that the flood tide commonly takes ten minutes less than the ebb tide.

379. As the different phenomena of the tides are chiefly distinguishable by the periods, or intervals of time in which they recur, it will be convenient to mark those periods by different names. Therefore, let the time of the apparent diurnal revolution of the Moon, viz. $24^h\ 50'$, be called a LUNAR DAY, and the 24th part of it be called A LUNAR HOUR. To this interval almost all the vicissitudes of the tides are most conveniently referred. Let the name TIDE DAY be given to the interval between two high waters, or two low waters, succeeding each other with the Moon nearly in the same position. This interval comprehends two complete tides, one of the full seas happening when the Moon is above the horizon, and the next when she is under the horizon. We shall also find it convenient to

distinguish these tides, by calling the first the SUPERIOR TIDE, and the other the INFERIOR TIDE. At new Moon they may be called the *Morning* and *Evening* tides.

380. 2. It is not only observed that we always have high water when the Moon is on some particular point of the compass (S. W. nearly) but also that the height of full sea from day to day has an evident reference to the phases of the Moon. At Brest, the highest tide is always about a day and a half after full or change. If it should happen that high water falls at the very time of new or full Moon, the third full sea after that one is the highest of all. This is called the SPRING-TIDE. Each succeeding full sea is less than the preceding, till we come to the third full sea after the Moon's quadrature. This is the lowest tide of all, and it is called NEAP-TIDE. After this, the tides again increase, till the next full or new Moon, the third after which is again the greatest tide.

381. The higher the tide of flood rises, the lower does the ebb tide generally sink on that day. The total magnitude of the tide is estimated by taking the difference between high and low water. As this is continually varying, the best way of computing its magnitude seems to be, to take the half sum of two succeeding tides. This must always give us a mean value for the tide whose full sea was in the middle. The medium spring-tide at Brest is about nineteen feet, and the neap-tide is about nine.

Here then we have a period of phenomena, the time of which is half of a lunar month. This period comprehends the most important changes, both in respect of magnitude, and of the hours of high and low water, and several modifications of both of those circumstances, such as the daily difference in height, or in time.

382. 3. There is another period, of nearly twice the same duration, which greatly modifies all those leading circumstances. This period has a reference to the distance of the Moon, and therefore depends on the Moon's revolution in

her orbit. All the phenomena are increased when the Moon is nearer to the Earth. Therefore the highest spring-tide is observed when the Moon is *in perigeo*, and the next spring-tide is the smallest, because the Moon is then nearly *in apogeo*. This will make a difference of 2¾ feet from the medium height of spring-tide at Brest, and therefore occasion a difference of 5½ between the greatest and the least. It is evident that as the perigean and apogean situation of the Moon may happen in every part of a lunation, the equation for the height of tide depending upon this circumstance may often run counter to the equation corresponding to the regular monthly series of tides, and will seemingly destroy their regularity.

383. 4. The variation in the Sun's distance also affects the tides, but not nearly so much as those in the distance of the Moon. In our winter, the spring-tides are greater than in summer, and the neap-tides are smaller.

384. 5. The declination, both of the Sun and Moon, affects the tides remarkably; but the effects are too intricate to be distinctly seen, till we perceive the causes on which they depend.

385. 6. All the phenomena are also modified by the latitude of the place of observation; and some phenomena occur in the high latitudes, which are not seen at all when the place of observation is on the equator. In particular, when the observer is in north latitude, and the Moon has north declination, that tide in which the Moon is above the horizon is greater than the other tide of the same day, when the Moon is below the horizon. It will be the contrary, if either the observer or the Moon (but not both) have south declination. If the polar distance of the observer be equal to the Moon's declination, he will see but one tide in the day, containing twelve hours flood and twelve hours ebb.

386. 7. To all this it must be added, that local circumstances of situation alter all the phenomena remarkably,

so as frequently to leave scarcely any circumstances of resemblance, except the order and periods in which the various phenomena follow one another.

We must now endeavour to account for these remarkable movements and vicissitudes in the waters of the ocean.

887. Since the phenomena of the planetary motions demonstrate that every particle of matter in this globe gravitates to the Sun, and since they are at various distances from his centre, it is evident that they gravitate unequally, and that, from this inequality, there must arise a disturbance of that equilibrium which terrestrial gravitation alone might produce. If this globe be supposed either perfectly fluid and homogeneous, or to consist of a spherical nucleus covered with a fluid, it is clear that the fluid must assume a perfectly spherical form, and that in this form alone every particle will be in equilibrio. But when we add to the forces now acting on the waters of the ocean, their unequal gravitation to the Sun, this equilibrium is disturbed, and the ocean cannot remain in this form. We may apply to the particles of the ocean every thing that we formerly said of the gravitation of the Moon to the Sun in the different points of her orbit; and the same construction in Fig. 31, that gave us a representation and measure of the forces which deranged the lunar motions, may be employed for giving us a notion of the manner in which the particles of water in the ocean are affected. The circle O B C A may represent the watery sphere, and M any particle of the water. The central particle E gravitates to the Sun with a force which may be represented by E S. The gravitation of the particle M must be measured by M G. This force M G may be conceived as compounded of M F, equal and parallel to E S, and of M H. The force M F occasions no alteration in the gravitation of M to the Earth, and M H is the only disturbing force. We found that this construction may be greatly simplified, and that M I may be substituted for M H without any sensible error, because

it never differs from it more than $\frac{1}{387}$. We therefore made E I, in Fig. 32, = 3 M N, and considered M I as the disturbing force. This construction is applicable to the present question, with much greater accuracy, because the radius of the Earth is but the sixtieth part of that of the Moon's orbit. This reduces the error to $\frac{1}{23220}$, a quantity altogether insensible.

388. Therefore let O A C B (Fig. 45.) be the terraqueous globe, and C S a line directed to the Sun, and B E A the section by that circle which separates the illuminated from the dark hemisphere. Let P be any particle, whether on the surface or within the mass. Let Q P N be perpendicular to the plane B A. Make E I = 3 P N, and join P I. P I is the disturbing force, when the line E S is taken to represent the gravitation of the particle E to the Sun. This force P I may be conceived to be compounded of two forces P E and P Q. P E tends to the centre of the Earth. P Q tends from the plane B A, or toward the Sun.

If this construction be made for every particle in the fluid sphere, it is evident that all the forces P E balance one another. Therefore they need not be considered in the present question. But the forces P Q evidently diminish the terrestrial gravitation of every particle. At C the force P Q acts in direct opposition to the terrestrial gravity of the particle. And, in the situation P, it diminshes the gravity of the particle as estimated in the direction P N. There is therefore a force acting in the direction N P on every particle in the canal P N. And this force is porportional to the distance of the particle from the plane B A (for P Q is always = 3 P N). Therefore the water in this canal cannot remain in its former position, its equilibrium being now destroyed. This may be restored, by adding to the column N P a small portion P *p*, whose weight may compensate the diminution in the weight of the column N P. A similar addition may be made to every such column perpendicular to the plane B E A. This being supposed, the spherical figure of the globe will be changed into that of an elliptical spheroid, having its axis in the line O C, and its poles in O and C.

Without making this addition to every column N P, we may understand how the *equilibrium* may be restored by the waters subsiding all around the circle whose section is B A, and rising on both sides of it. For it was shewn (320.) that in a fluid elliptical spheroid of gravitating matter, the gravitation of any particle P to all the other particles may be resolved into two forces P N and P M perpendicular to the plane B A and to the axis O C, and proportional to P N and P M; and that if the forces be really in this proportion, the whole will be in equilibrio, provided that the whole forces at the poles and equator are inversely as the diameters O C and B A. Now this may be the case here. For the forces superadded to the terrestrial gravitation of any particle are, *1st*, A force P E proportional to P E. When this is resolved into the directions P N and P M, the forces arising in this resolution are as P N and P M, and therefore in the due proportion: *2d*, the force P Q, which is also as P N. It is evident therefore that this mass may acquire such a protuberancy at O and C, that the force at O shall be to the force at B as B A to O C, or as E A to E C. We are also taught in sect. 341. what this protuberance must be. It must be such that four times the mean gravity of a particle on the surface is to five times the disturbing force at O or C as the diameter B A is to the excess of the diameter O C. This ellipticity is expressed by the same formula as in the former case, viz. $\frac{x}{r} = \frac{4c}{5g}, = \frac{\text{E C} - \text{E A}}{\text{E C}}$.

389. Thus we have discovered that, in consequence of the unequal gravitation of the matter in the Earth to the Sun, the waters will assume the form of an oblong elliptical spheroid, having its axis directed to the Sun, and its poles in those points of the surface which have the Sun in

the zenith and nadir. There the waters are highest above the surface of a sphere of equal capacity. All around the circumference B E A, the waters are below the natural level. A spectator placed on this circumference sees the Sun in the horizon.

We can tell exactly what this proturberance E O — E A must be, because we know the proportions of all the forces. Let W represent the terrestrial gravitation, or the weight of the particle C, and G the gravitation of the same particle to the Sun, and let F be the disturbing force acting on a particle at C or at O, and therefore $= 3\,CE$. Let S and E be the quantity of matter in the Sun and in the Earth.

Then (Fig. 31.) $F : G = 3\,CE : CG$

$$G : W = \frac{S}{CS^2} : \frac{E}{CE^2} \text{ (222.)}$$

therefore $$F : W = \frac{3\,CE \times S}{CS^2} : \frac{CG \times E}{CE^2} =$$

$\frac{3\,S}{CS^2 \times CG} : \frac{E}{CE^3}$. But, because $CS^2 : ES^2 = ES : CG$, we have $CS^2 \times CG = ES^2 \times ES, = ES^3$. Therefore $F : W = \frac{3\,S}{ES^3} : \frac{E}{EC^3}$. Now $E : S = 1 : 338343$, and $EC : ES = 1 : 23668$. This will give $\frac{3\,S}{ES^3} : \frac{E}{EC^3} = 1 : 12778541, = F : W$.

Finally, $4\,W : 5\,F = CE : CE - AE$. We shall find this to be nearly 24½ inches.

390. Such is the figure that this globe would assume, had it been originally fluid, or a spherical nucleus covered with a fluid of equal density. The two summits of the watery spheroid would be raised about two feet above the equator or place of greatest depression.

But the Earth is an oblate spheroid. If we suppose it covered, to a moderate depth, with a fluid, the waters would acquire a certain figure, which has been considered already. Let the disturbing force of the Sun act on this figure. A *change* of figure must be produced, and the waters under the Sun, and those in the opposite parts, will be elevated above their natural surface, and the ocean will be depressed on the circumference B E A. It is plain, that this *change* of figure will be almost the same in every place as if the Earth were a sphere. For the difference between the *change* produced by the Sun's disturbing force on the figure of the fluid sphere or fluid spheroid, arises solely from the difference in the gravitation of a particle of water to the sphere and to the spheroid. This difference, in any part of the surface, is exceedingly small, not being $\frac{1}{300^2}$ of the whole gravitation. The difference, therefore, in the *change* produced by the Sun cannot be $\frac{1}{300^2}$ of the whole change. Therefore, since it is from the *proportion* of the disturbing force to the force of gravity that the ellipticity is determined, it follows, that the *change* of figure is, to all sense, the same, whether the Earth be a sphere or a spheroid whose eccentricity is less than $\frac{1}{231}$.

Let us suppose, for the present, that the watery spheroid always has that form which produces an equilibrium in all its particles. This cannot ever be the case, because some time must elapse before an accelerating force can produce any finite change in the disposition of the waters. But the contemplation of this figure gives us the most distinct notion of the forces that are in action, and of their effects; and we can afterwards state the difference that must obtain, because the figure is not completely attained.

Supposing it really attained, it follows, that the ocean will be most elevated in those places which have the Sun in the zenith or nadir, and most depressed in those places where the Sun is seen in the horizon. While the Earth turns round its axis, the pole of the spheroid keeps still toward the Sun, as if the waters stood still, and the solid nucleus turned round under it. The phenomena may perhaps be easier conceived by supposing the Earth to remain at rest, and the Sun to revolve round it in 24 hours from east to west. The pole of the spheroid follows him, as the card of a mariner's compass follows the magnet; and a spectator attached to one part of the nucleus will see all the vicissitudes of the tide. Suppose the Sun in the equinox, and the observer also on the Earth's equator, and the Sun just rising to him. The observer is then in the lowest part of the watery spheroid. As the Sun rises above the horizon, the water also rises; and when the Sun is in the zenith, the pole of the spheroid has now reached the observer, and the water is two feet deeper than it was at sun-rise. The Sun now approaching the western horizon, and the pole of the ocean going along with him, the observer sees the water subside again, and at sun-set it is at the same level as at sunrise. As the Sun continues his course, though unseen, the opposite pole of the ocean now advances from the east, and the observer sees the water rise again by the same degrees as in the way of forming a good guess of the state of the tide is to morning, and attain the height of two feet at midnight, and again subside to its lowest level at six o'clock in the following morning.

Thus, in 24 hours, he has two tides of flood and two ebb tides; high water at noon and midnight, and low water at six o'clock morning and evening. An observer, not in the equator, will see the same *gradation* of phenomena at the same hours; but the rise and fall of the water will not be so considerable, because the pole of the spheroid passes his meridian at some distance from him. If the spectator is in the pole of the Earth, he will see no change, because he is always in the lowest part of the watery spheroid.

From this account of the simplest case, we may infer, that the depth of the water, or its change of depth, depends entirely on the shape of the spheroid, and the place of it occupied by the observer.

391. To judge of this with accuracy, we must take notice of some properties of the ellipse which forms the meridian of the watery spheroid. Let A E *a* Q (Fig. 46.) represent this elliptical spheroid, and let B E *b* Q be the inscribed sphere, and A G *a g* the circumscribed sphere. Also let D F *d f* be the sphere of equal capacity with the spheroid. This will be the natural figure of the ocean, undisturbed by the gravitation to the Sun.

In a spheroid like this, so little different from a sphere, the elevation A D of its summit above the equally capacious sphere is very nearly double of the depression F E of its equator below the surface of that sphere. For spheres and spheroids, being equal to $\frac{2}{3}$ of the circumscribing cylinders, are in the ratio compounded of the ratio of their equators and the ratio of their axes. Therefore, since the sphere D F *d f* is equal to the spheroid A E *a* Q, we have $CF^2 \times CD = CE^2 \times CA$, and $CE^2 : CF^2 = CD : CA$. Make $CE : CF = CF : Cx$, then $CE : Cx = CD : CA$, and $CE : Ex = CD : DA$, and $CE : CD = Ex : DA$. Now C E does not differ sensibly from C D (only eight inches in near 4000 miles), therefore E x may be accounted equal to D A But E x is not sensibly different from twice E F. Therefore the proposition is manifest.

392. In such an elliptical spheroid, the elevation I L of any point I above the inscribed sphere is proportional to the square of the cosine of its distance from the pole A, and the depression K I of this point below the surface of the circumscribed sphere is as the square of the sine of its distance from the pole A. Draw through the point I, H I M perpendicular to C A, and I *p* N perpendicular to C E. The triangles C I N and *p* I L are similar.

Therefore $p\mathrm{I} : \mathrm{IL} = \mathrm{CI} : \mathrm{IN}, = \mathrm{rad.} : \mathrm{cos.\ ICA}$
but by the ellipse $\mathrm{AB} : p\mathrm{I} = \mathrm{AC} : \mathrm{IN}, = \mathrm{rad.} : \mathrm{cos.\ ICA}$
therefore $\mathrm{AB} : \mathrm{IL} = \mathrm{rad.}^2 : \mathrm{cos.}^2\ \mathrm{ICA}$
and IL is always in the proportion of $\mathrm{cos.}^2$, ICA, and is $= \mathrm{AB} \times \mathrm{cos.}^2$, ICA, radius being $= 1$.
In like manner, $\mathrm{HI} : \mathrm{IK} = \mathrm{CI} : \mathrm{IM} = \mathrm{rad.} : \mathrm{sin.\ ICA}$
and $\mathrm{GE} : \mathrm{HI} = \mathrm{EC} : \mathrm{IM} = \mathrm{rad.} : \mathrm{sin.\ ICA}$
therefore $\mathrm{GE} : \mathrm{KI} = \mathrm{rad.}^2 : \mathrm{sin.}^2\ \mathrm{ICA}$
and KI is $= \mathrm{AB} \times \mathrm{sin.}^2\ \mathrm{ICA}$.

393. We must only know the elevations and depressions in respect of the natural level of the undisturbed ocean. This elevation for any point i is evidently $il - ml = \mathrm{AB} \times \mathrm{cos.}^2\, i\mathrm{CA} - \frac{1}{3}\,\mathrm{AB} \times \overline{\mathrm{cos.}^2\, i\mathrm{CA} - \frac{1}{3}}$, and the depression nr of a point r is $kr - kn = \mathrm{AB} \times \mathrm{sin.}^2\, r\mathrm{CA} - \frac{2}{3}\,\mathrm{AB}$, $= \mathrm{AB} \times \overline{\mathrm{sin.}^2\, r\mathrm{CA} - \frac{2}{3}}$.

It will be convenient to employ a symbol for expressing the whole difference AB or GE between high and low water produced by the action of the Sun. Let it be expressed by the symbol S. Also, let the angular distance from the summit, or from the Sun's place, be x.

The elevation mi is $= \mathrm{S} \times \mathrm{cos.}^2\, x - \frac{1}{3}\,\mathrm{S}$.
The depression nr is $= \mathrm{S} \times \mathrm{sin.}^2\, x - \frac{2}{3}\,\mathrm{S}$.

394. The spheroid intersects the equicapacious sphere in a point so situated, that $\mathrm{S} \times \mathrm{cos.}^2\, x - \frac{1}{3}\,\mathrm{S} = 0$, that is, where $\mathrm{cos.}^2\, x = \frac{1}{3}$. This is 54° 44'. from the pole of the spheroid, and 35° 16' from its equator, a situation that has several remarkable physical properties. We have already seen, (328.) that on this part of the surface the gravitation is the same as if it were really a perfect sphere.

395. The ocean is made to assume an eccentric form, not only by the unequal gravitation of its waters to the Sun, but also by their much more unequal gravitation to the Moon; and although her quantity of matter is very small indeed, when compared with the Sun, yet being almost 400 times nearer, the inequality of gravitation is increased almost 400×400×400 times, and may therefore

produce a sensible effect.* We cannot help presuming that it does, because the vicissitudes of the tides have a most distinct reference to the position of the Moon. Without going over the same ground again, it is plain that the waters will be accumulated under the Moon, and in the opposite part of the spheroid, in the same manner as they are affected by the Sun's action.

Therefore, let M represent the elevation of the pole of the spheroid above the equicapacious sphere that is produced by the unequal gravitation to the Moon, and let y be the angular distance of any part of this spheroid from its pole. We shall then have

The elevation of any point $= M \times \cos.^2 y - \frac{1}{3} M$.
The depression $= M \times \sin.^2 y - \frac{2}{3} M$.

296. In consequence of the simultaneous gravitation to both luminaries, the ocean must assume a form differing from both of these regular spheroids. It is a figure of difficult investigation; but all that we are concerned in may be determined with sufficient accuracy by means of the following considerations:

We have seen, that the *change* of figure induced on the spheroidal ocean of the revolving globe is nearly the same as if it were induced on a perfect sphere. Much more securely may we say, that the change of figure, induced on the ocean already disturbed by the Sun, is the same that the Moon would have occasioned on the undisturbed revolving spheroid. We may therefore suppose, without any sensible error, that the change produced in any part of the ocean by the joint action of the two luminaries is the sum or the difference of the changes which they would have produced separately.

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* The distance of the Sun being about 392 times that of the Moon, and the quantity of matter in the Sun about 338000 times that in the Earth, if the quantity of matter in the Moon were equal to that in the Earth, her accumulating force would be 178 times greater than that of the Sun. We shall see that it is nearly $2\frac{1}{3}$ times greater. From which we should infer, that the quantity of matter in the Moon is nearly $\frac{1}{71}$ of that in the Earth. This seems the best information that we have on this subject.

397. Therefore, since the poles of both spheroids are in those parts of the ocean which have the Sun and the Moon in the zenith, it follows, that if x be the zenith distance of the Sun from any place, and y the zenith distance of the Moon, the elevation of the waters above the natural surface of the undisturbed ocean will be $S \times \cos.^2 x - \frac{1}{3} S + M \times \cos.^2 y - \frac{1}{3} M$. And the depression in any place will be $S \times \sin.^2 x - \frac{2}{3} S + M \times \sin.^2 y - \frac{2}{3} M$. This may be better expressed as follows:

$$\text{Elevation} \quad = S \times \cos.^2 x + M \times \cos.^2 y - \tfrac{1}{3}\,\overline{S + M}.$$

$$\text{Depression} \quad = S \times \sin.^2 x + M \times \sin.^2 y - \tfrac{2}{3}\,\overline{S + M}.$$

398. Suppose the Sun and Moon to be in the same part of the heavens. The solar and lunar tides will have the same axes, poles, and equator, the gravitations to each conspiring to produce a great elevation at the combined pole, and a great depression all round the common equator. The elevation will be $\frac{2}{3}\,\overline{S + M}$, and the depression will be $\frac{1}{3}\,\overline{S + M}$. Therefore the elevation above the inscribed sphere (or rather the spheroid similar and similarly placed with the natural revolving spheroid) will be $\overline{S + M}$.

399. Suppose the Moon in quadrature in the line EDM (Fig. 47.) It is plain, that one luminary tends to produce an elevation above the equicapacious sphere AOBC, in the point of the ocean A immediately under it, where the other tends to produce a depression, and therefore their forces counteract each other. Let the Sun be in the line ES.

The elevation at $S = S - \frac{1}{3}\,\overline{S + M}, = \frac{2}{3} S - \frac{1}{3} M$.

The depression at $M = S - \frac{2}{3}\,\overline{S + M}, = \frac{1}{3} S - \frac{2}{3} M$.

The elevation at S above the inscribed spheroid = S — M.
The elevation at M above the same = M — S.

Hence it is evident, that there will be high water at M or at S, when the Moon is in quadrature, according as the accumulating force of the Moon exceeds or falls short of that of the Sun. Now, it is a matter of observation, that when the Moon is in quadrature, it is high water in the open seas under the Moon, and low water under the Sun, or nearly so. This observation confirms the conclusion drawn from the nutation of the Earth's axis, that the disturbing force of the Moon exceeds that of the Sun. This criterion has some uncertainty, owing to the operation of local circumstances, by which it happens, that the summit of the water is never situated either under the Sun or under the Moon. But even in this case, we find that the high water is referable to the Moon, and not to the Sun. It is always six hours of the day later than the high water at full or change. This corresponds with the elongation of the Moon six hours to the eastward. The phenomena of the tides shew further, that, at this time, the waters under the Sun are depressed below the natural surface of the ocean. This shews that M is more than twice as great as S.

400. When the Moon has any other position besides these two, the place of high water must be some intermediate position. It must certainly be in the great circle passing through the simultaneous places of the two luminaries. As the place and time of high and low water, and the magnitude of the elevation and depression, are the most interesting phenomena of the tides, they shall be the principal objects of our attention.

The place of high water is that where the sum of the elevations produced by both luminaries above the natural surface of the ocean is a maximum. And the place of low water, in the great circle passing through the Sun and Moon, is that where the depression below the natural level of the ocean is a maximum. Therefore, in order to have the place of high water, we must find where $S \times \cos.^2 x + M \times \cos.^2 y - \frac{1}{3}\overline{S+M}$ is a maximum. Or, since $\frac{1}{3}\overline{S+M}$ is a constant quantity, we must find where $S \times \cos.^2 x + M \times \cos.^2 y$ is a maximum. Now, accounting the tabular sines and cosines as fractions of radius, $= 1$, we have

$$\text{Cos.}^2 x = \tfrac{1}{2} + \tfrac{1}{2} \cos.\, 2x$$

and $$\text{Cos.}^2 y = \tfrac{1}{2} + \tfrac{1}{2} \cos.\, 2y.$$

For let A B S D (Fig. 48.) be a circle, and A S, B D two diameters crossing each other at right angles. Describe on the semidiameter C S the small circle C *m* S *h*, having its centre in *d*. Let H C make any angle x with C S, and let it intersect the small circle in *h*. Draw *d h*, S *h*, producing S *h* till it meet the exterior circle in *s*, and join A *s*, C *s*. Lastly, draw *h o* and *s r* perpendicular to C S.

S *h* is perpendicular to C *h*, and $CS : Ch = \text{rad.} : \cos.\, HCS$, and $CS : Co = R^2 : \cos.^2 HCS$. The angle S C *s* is evidently $= 2\, SCH = S\,d\,h$ and $A\,r = 2\,C\,o$. Now, if CS be $= 1$; $C\,r = \cos.^2 2x$; $A\,r = 1 + \cos.\, 2x$. Therefore $C\,o = \frac{1}{2} + \frac{1}{2}\cos.\, 2x$. In like manner, $\cos.^2 y = \frac{1}{2} + \frac{1}{2}\cos.\, 2y$

Therefore we must have $\frac{S}{2} + \frac{S \times \cos.\, 2x}{2} + \frac{M}{2} + \frac{M \times \cos.\, 2y}{2}$ a maximum; or, neglecting the constant quantities $\frac{S}{2}, \frac{M}{2}$, and the constant divisor 2, we must have $S \times \cos.\, 2x + M \times \cos.\, 2y$ a maximum.

Let A B S D (fig. 48.) be now a great circle of the Earth, passing through those points S and M of its surface which have the Sun and the Moon in the zenith. Draw the diameter S C A, and cross it at right angles by B C D. Let S *d* be to *d a* as the accumulating force of the Moon to the accumulating force of the Sun, that is, as M to S, which proportion we suppose known. Draw C M in the direction of the Moon's place. It will cut the small circle in some point m. Join $m\,a$. Let H be any point of the surface of the ocean. Draw C H, cutting the small circle in h. Draw the diameter $h\,d\,h'$. Draw $m\,t$ and $a\,x$ perpendicular to $h\,h'$, and $a\,y$ parallel to $h\,h'$, and join $m\,d$. Also draw the chords $m\,h$ and $m\,h'$.

In this construction, $m\,d$ and $d\,a$ represent M and S, the angle $\mathrm{M\,C\,H} = y$, and $\mathrm{S\,C\,H} = x$. It is farther manifest that the angle $m\,d\,h = 2\,m\,\mathrm{C}\,h, = 2\,y$, and that $d\,t = \mathrm{M} \times \cos.\,2\,y$. In like manner, $h\,d\,\mathrm{S} = 2\,\mathrm{H\,C\,S}, = 2\,x$, and $d\,x = d\,a \times \cos.\,2\,x, = \mathrm{S} \times \cos.\,2\,x$. Therefore $t\,x = \mathrm{S} \times \cos.\,2\,x + \mathrm{M} \times \cos.\,2\,y$. Moreover $t\,x = a\,y$, and is a maximum when $a\,y$ is a maximum. This must happen when $a\,y$ coincides with $a\,m$, that is, when $h\,d$ is parallel to $a\,m$.

Hence may be derived the following construction:

Let A M S (fig. 49.) be, as before, a great circle, whose plane passes through the Sun and the Moon. Let S and M be those points which have the Sun and the Moon in the zenith. Describe, as before, the circle C m S, cutting C M in m. Make $\mathrm{S}\,d : d\,a = \mathrm{M} : \mathrm{S}$, and join $m\,a$. Then, for the place of high water, draw the diameter $h\,d\,h'$ parallel to $m\,a$, cutting the circle C m S in h. Draw C h H, cutting the surface of the ocean in H and H′. Then H and H′ are the places of high water. Also draw C h', cutting the surface of the ocean in L and L′. L and L′ are the places of low water in this circle.

For, drawing $m\,t$ and $a\,x$ perpendicular to $h\,h'$, it is plain that $t\,x = \mathrm{M} \times \cos.\,2\,y + \mathrm{S} \times \cos.\,2\,x$. And what was just now demonstrated shews that $t\,x$ is in its maximum state. Also, if the angle $\mathrm{L\,C\,S} = u$, and $\mathrm{L\,C\,M} = z$, it is evident that $d\,x = \mathrm{S} \times \cos.\,a\,d\,x, = \mathrm{S} \times \cos.\,h'\,d\,\mathrm{S}, = \mathrm{S} \times \cos.\,2\,h'\,\mathrm{C\,S}, = \mathrm{S} \times \cos.\,2\,\mathrm{L\,C\,S}, = \mathrm{S} \times \cos.\,2\,u$; and, in like manner, $t\,d = \mathrm{M} \times \cos.\,2\,z$; and therefore $t\,x = \mathrm{S} \times \cos.\,2\,u + \mathrm{M} \times \cos.\,2\,z$, and it is a maximum.

It is plain, independent of this construction, that the places of high and low water are 90^0 asunder; for the two hemispheres of the ocean must be similar and equal, and the equator must be equidistant from its poles.

401. Draw df perpendicular to ma. Then, if dS be taken to represent the whole tide produced by the Moon, that is, the whole difference in the height of high and low water, ma will represent the compound tide at H, or the difference between high and low water corresponding to that situation of the place H with respect to the Sun and Moon. mf will be the part of it produced by the Moon, and af the part produced by the Sun.

For the elevation at H above the natural level is $S \times \overline{\cos.^2 x - \frac{1}{3}} + M \times \overline{\cos.^2 y - \frac{1}{3}}$, and the depression below it at L is $\overline{S \times \sin.^2 u - \frac{2}{3}} + M \times \overline{\sin.^2 z - \frac{2}{3}}$. But $\sin.^2 u = \cos.^2 x$, and $\sin.^2 z = \cos.^2 y$. Therefore the depression at L is $S \times \overline{\cos.^2 x - \frac{2}{3}} + M \times \overline{\cos.^2 y - \frac{2}{3}}$. The sum of these makes the whole difference between high and low water, or the whole tide. Therefore the tide is $= S \times \overline{2 \cos.^2 x - 1} + \overline{M \times 2 \cos.^2 y - 1}$. But $2 \cos.^2 x - 1 = \cos. 2x$, and $2 \cos.^2 y - 1 = \cos. 2y$. Therefore the tide $= S \times \cos. 2x + M \times \cos. 2y$. Now it is plain that $mf = md \cos. dmf$, and that the angle $dmf = mdh, = 2mCh, = 2y$. Therefore $md \times \cos. dmf = M \times \cos. 2y$. In like manner $af = S \times \cos. 2x$.

The point a must be within or without the circle CmS, according as M is greater or less than S, that is, according as the accumulating force of the Moon is greater or less than that of the Sun. It appears also that, in the first case, H will be nearer to M, and in the second case, it will be nearer to S.

Thus have we given a construction that seems to express all the phenomena of the tides, as they will occur to a spectator placed in the circle passing through those points which have the Sun and Moon in the zenith. It marks the distance of high water from those two places, and therefore, if the luminaries are in the equator, it marks the time that will elapse between the passage of the Sun or Moon over the meridian, and the moment of high water. It also expresses the whole height of the tide of that day. And, as the point H may be taken without any reference to high water, we shall then obtain the state of the tide for that hour, when it is high water in its proper place H. By considering this construction for the different relative positions of the Sun and Moon, we shall obtain a pretty distinct notion of the series of phenomena which proceed in regular order during a lunar month.

402. To obtain the greater simplicity in our first and most general conclusions, we shall first suppose both luminaries in the equator. Also, abstracting our attention from the annual motion of the Sun, we shall consider only the relative motion of the Moon in her synodical revolution, stating the phenomena as they occur when the Moon has got a certain number of degrees away from the Sun; and we shall always suppose that the watery spheroid has attained the form suited to its equilibrium in that situation of the two luminaries. The conclusions will frequently differ much from common observation. But we shall afterwards find their agreement very satisfactory. The reader is therefore expected to go along with the reasoning employed in this discussion, although the conclusions may frequently surprise him, being very different from his most familiar observations.

403. 1. At new and full Moon, we shall have high water at noon, and at midnight, when the Sun and Moon are on the meridian. For in this case C M, *a m*, C S, *d h*, C H, all coincide.

404. 2. When the Moon is in quadrature in B, the place of high water is also in B, under the Moon, and this happens when the Moon is on the meridian. For when M C is perpendicular to C S, the point *m* coincides with C, *a m* with *a* C, and *d h* with *d* C.

405. 3. While the Moon passes from a syzigy to the

next quadrature, the place of high water follows the Moon's place, keeping to the westward of it. It overtakes the Moon in the quadrature, gets to the eastward of the Moon, (as it is represented at $M^2 H^2$, by the same construction), preceding her while she passes forward to the next syzigy, in A, where it is overtaken by the Moon's place. For while M is in the quadrant S B, or A D, the point h is in the arch S m. But when M is in the quadrant B A or D S, h^2 is without or beyond the arch S m^2 (counted *eastward* from from S.) Therefore, during the first and third quarters of the lunation, we have high water after noon or midnight, but before the Moon's southing. But in the second and fourth quarters, it happens after the Moon's southing.

406. 4. Since the place of high water coincides with the Moon's place both in syzigy and the following quadrature, and in the interval is between her and the Sun, it follows that it must, during the first and third quarters, be gradually left behind, for a while, and then must gain on the Moon's place, and overtake her in quadrature. There must therefore be a certain greatest distance between the place of the Moon and that of high water, a certain maximum of the angle M C H. This happens when H′ C S is exactly 45°. For then $h' d$ S is 90°, $m' a$ is perpendicular to a S, and the angle $a\,m'\,d$ is a maximum. Now $a\,m'\,d = m'\,d\,h'$, $= 2\,y'$.

407. When things are in this state, the motion of high water, or its separation from the Sun to the eastward, is equal to the Moon's easterly motion. Therefore, at new and full Moon, it must be slower, and at the quadratures it must be swifter. Consequently, when the Moon is in the octant, 45° from the Sun, the interval between two successive southings of the Moon, which is always $24^h\ 50'$ nearly, must be equal to the interval of the two concomitant or superior high waters, and each tide must occupy $12^h\ 25'$, the half of a lunar day. But at new or full Moon, the interval between the two successive high waters

must be less than $12^h\ 25'$, and in the quadratures it must be more.

408. The tide day must be equal to the lunar day only when the high water is in the octants. It must be shorter at new and full Moon, and while the Moon is passing from the second octant to the third, and from the fourth to the first. And it must exceed a lunar day while the Moon passes from the first octant to the second, and from the third to the fourth. The tide day is always greater than a solar day, or twenty-four hours. For, while the Sun makes one round of the earth, and is again on the meridian, the Moon has got about 13° east of him, or S M is nearly 13° and S H is nearly 9°, so that the Sun must pass the meridian about 35 or 36 minutes before it is high water. Such is the law of the daily retardation called the priming or lagging of the tides. At new and full Moon it is nearly 35', and at the quadratures it is 85', so that the tide day at new and full Moon is $24^h\ 35'$, and in the quadratures it is $25^h\ 25'$ nearly.

Our construction gives us the means of ascertaining this circumstance of the tides, or interval between two succeeding full seas, and it may be thus expressed:

409. The synodical motion of the Moon is to the synodical motion of the high water as $m\,a$ to $m\,f$. For, take a point u very near to m. Draw $u\,a$ and $u\,d$, and draw $d\,i$ parallel to $a\,u$, and with the centre a, and distance $a\,u$, describe the arch $u\,v$, which may be considered as a straight line perpendicular to $m\,a$. Then $u\,m$ and $i\,h$ are respectively equal to the motions of M and H (though they subtend twice the angles. The angles $a\,u\,v$, $d\,u\,m$, are equal, being right angles. Therefore $m\,u\,v = a\,u\,d, = a\,m\,d$, and the triangles $m\,u\,v$, $d\,m\,f$, are similar, and the angles $u\,a\,m$, $i\,d\,h$ are equal, and therefore

$$u\,v : i\,h = m\,a : h\,d, = m\,a : m\,d$$
$$u\,m : u\,v = m\,d : m\,f$$
therefore $u\,m : i\,h = m\,a : m\,f$

When m coincides with S, that is, at new or full Moon,

$m\,a$ coincides with S a, and $m\,f$ with S d. But when m coincides with C, that is, in the quadratures, $m\,a$ coincides with C a, and $m\,f$ with C d.

410. Hence it is easy to see, that the retardation of the tides at new and full Moon is to the retardation in the quadratures as C a to S a, that is as M + S to M — S.

When the high water is in the octant, $m\,a$ is perpendicular to S a, and therefore a and f coincide, and the synodical motion of the Moon and of high water are the same, as has been already observed.

Let us now consider the elevations of the water, and the magnitude of the tide, and its gradual variation in the course of a lunation. This is represented by the line $m\,a$.

411. This series of changes is very perceptible in our construction. At new and full Moon, $m\,a$ coincides with S a; and in the quadratures, it coincides with C a. Therefore, the spring-tide is to the neap-tide as S a to C a, that is, as M + S to M — S. From new or full Moon the tide gradually lessens to the time of the quadrature. We also see that the Sun contributes to the elevation by the part $a\,f$, till the high water is in the octants, for the point f lies between m and a. After this, the action of the Sun diminishes the elevation, the point f then lying beyond a.

412. The momentary change in the height of the whole tide, that is, in the difference between the high and low water, is proportional to the sine of twice the arch M H. It is measured by $d\,f$ in our construction. For, let $m\,u$ be a given arch of the Moon's synodical motion, such as a degree. Then $m\,v$ is the difference between the tides $m\,a$ and $u\,a$, corresponding to the constant arch of the Moon's momentary elongation from the Sun. The similarity of the triangles $m\,u\,v$ and $m\,d\,f$ gives us $m\,u : m\,v = m\,d : d\,f$. Now $m\,u$ and $m\,d$ are constant. Therefore $m\,v$ is proportional to $d\,f$, and $m\,d : d\,f =$ rad. : sin. $d\,m\,f$, $=$ sin. $m\,d\,h$, $=$ sin. 2 M C H.

Hence it follows that the diminution of the tides is most rapid when the high water is in the octants. This will be found to be the difference between the twelfth and thirteenth tides, counted from new or full Moon, and between the seventh and eighth tides after the quadratures. If $m\,a$ be taken = ½ the Moon's daily elongation from the Sun, which is 6° 30′ nearly, the rule will give, with sufficient accuracy, ½ the difference between the two superior or the two inferior tides immediately succeeding. It does not give the difference between the two immediately succeeding tides, because they are alternately greater and lesser, as will appear afterwards.

413. Having thus given a representation to the eye of the various circumstances of these phenomena in this simple case, it would be proper to shew how all the different quantities spoken of may be computed arithmetically. The simplest method for this, though perhaps not the most elegant, seems to be the following:

In the triangle $m\,d\,a$, the two sides $m\,d$ and $d\,a$ are given, and the contained angle $m\,d\,a$, when the proportion of the forces M and S, and the Moon's elongation M C S are given. Let this angle $m\,d\,a$ be called a. Then make $\mathrm{M}+\mathrm{S}:\mathrm{M}-\mathrm{S}=\tan.\,a:\tan.\,b$. Then $y=\frac{a-b}{2}$, and $x=\frac{a+b}{2}$.

For $\mathrm{M}+\mathrm{S}:\mathrm{M}-\mathrm{S}=m\,d+d\,a:m\,d-d\,a,=\tan.\,\frac{m\,a\,d+a\,m\,d}{2}:\tan.\,\frac{m\,a\,d-a\,m\,d}{2}=\tan.\,\frac{2\,x+2\,y}{2}:\tan.\,\frac{2\,x-2\,y}{2},=\tan.\,\overline{x+y}:\tan.\,\overline{x-y}=\tan.\,a:\tan.\,b$. Now $\overline{x+y}+\overline{x-y}=2\,x$ and $\overline{x+y}-\overline{x-y}=2\,y$. Therefore $a+b=2\,x$ and $a-b=2\,y$, and $x=\frac{a+b}{2}$, and $y=\frac{a-b}{2}$.

414. It is of peculiar importance to know the greatest separation of the high water from the Moon. This happens when the high water is in the octant. In this situa-

tion it is plain that $m'\,d : d\,a$, that is, $M : S$, $=$ rad. : sin. $d\,m'\,a$, $=$ rad. : sin. $2\,y'$, and therefore sin. $2\,y' = \frac{S}{M}$. Hence $2\,y$ and y' are found.

415. It is manifest that the applicability of this construction to the explanation of the phenomena of the tides depends chiefly on the proportion of $S\,d$ to $d\,a$, that is, the proportion of the accumulating force of the Moon to that of the Sun. This constitutes the species of the triangle $m\,d\,a$, on which every quantity depends. The question now is, What is this proportion? Did we know the quantity of matter in the Moon, it would be decided in a minute. The only observation that can give us any information on this subject is the nutation of the Earth's axis. This gives at once the proportion of the disturbing forces. But the quantities observed, the deviation of the Earth's axis from its uniform conical motion round the pole of the ecliptic, and the equation of the precession of the equinoctial points are much too small for giving us any precise knowledge of this ratio.

Fortunately, the tides themselves, by the modification which their phenomena receive from the comparative magnitude of the forces in question, give us means of discovering the ratio of S to M. The most obvious circumstance of this nature is the magnitude of the spring and neap-tides. Accordingly, this was employed by Newton in his theory of the tides. He collected a number of observations made at Bristol and at Plymouth, and, stating the spring-tide to the neap-tide as $M + S$ to $M - S$, he said that the force of the Moon in raising the tide is to that of the Sun nearly as $4\frac{1}{2}$ to 1. But it was soon perceived that this was a very uncertain method. For there are scarcely any two places where the proportion between the spring-tide and the neap-tide is the same, even though the places be very near each other. This extreme discrepancy, while the proportion was observed to be invariable for any individual place, shewed that it was not the theory that was in fault, but that the local circumstances of situation were such as affected very differently tides of different magnitudes, and thus changed their proportion. It was not till the noble collection of observations was made at Brest and Rochefort that the philosopher could assort and combine the immense variety of heights and times of the tides, so as to throw them into classes to be compared with the aspect of the Sun and Moon according to the Newtonian theory. M. Cassini, and, after him, M. Daniel Bernoulli, made this comparison with great care and discernment; and on the authority of this comparison, M. Bernoulli has founded the theory and explanation contained in his excellent Dissertation on the Tides, which shared with M'Laurin and Euler the prize given by the Academy of Paris in 1740.

M. Bernoulli employs several circumstances of the tides for ascertaining the ratio of M to S. He employs the law of the retardation of the tides. This has great advantages over the method employed by Newton. Whatever are the obstructions or modifications of the tides, they will operate equally, or nearly so, on two tides that are equal, or nearly equal. This is the case with two succeeding tides of the same kind.

The Moon's mean motion from the Sun, in time, is about 50½ minutes in a day. The smallest retardation, in the vicinity of new and full Moon, is nearly 35′, wanting 15½ of the Moon's retardation. Therefore, by art. 412,

$$M : S = 35 : 15\tfrac{1}{2}, = 5 : 2\tfrac{1}{4} \text{ nearly.}$$

The longest tide-day about the quadratures is 25h 25′, exceeding a solar day 85′, and a lunar day 34½. Therefore

$$M : S = 85 : 34\tfrac{1}{2}, = 5 : 2\tfrac{1}{30} \text{ nearly.}$$

The proportion of M to S may also be inferred by a direct comparison of the tide-day at new Moon and in the quadratures.

$$35 : 85 = M - S : M + S. \quad \text{Therefore}$$
$$M : S = \frac{85+35}{2} : \frac{85-35}{2}, = 5 : 2\tfrac{1}{12}.$$

It may also be discovered by observing the greatest separation of the place of high water from that of the Moon, or the elongation of the Moon when the tide-day and the lunar day are equal. In this case y is observed to be nearly 12° 30′. Therefore $\frac{S}{M} = \sin. 25°$, and $M : = 5 : 2\tfrac{1}{8}$ nearly.

Thus it appears that all these methods give nearly the same result, and that we may adopt 5 to 2 as the ratio of the two disturbing forces. This agrees extremely well with the phenomena of nutation and precession.

Instead of inferring the proportion of M to S, from the quantity of matter in the Moon, deduced from the phenomena of nutation, as is affected by D'Alembert and La Place, I am more disposed to infer the mass of the Moon from this determination of M : S, confirmed by so many coincidences of different phenomena. Taking 5 : 2,18 as the mean of those determinations, and employing the analogy in sect. 227, we obtain for the quantity of matter in the Moon nearly $\frac{1}{70}$, the Earth being 1.

If the forces of the two luminaries were equal, there would be no high and low water in the day of quadrature. There would be an elevation above the inscribed spheroid of $\frac{1}{2}\,\overline{M+S}$ all round the circumference of the circle passing through the Sun and Moon, forming the ocean into an oblate spheroid.

416. Since the gravitation to the Sun alone produces an elevation of 24½ inches, the gravitation to the Moon will raise the waters 58 inches; the spring-tide will be 24½ + 58, or 82½ inches, and the neap-tide 33½ inches.

417. The proportion now adopted must be considered as that corresponding to the mean intensity of the accumulating forces. But this proportion is by no means constant,

by reason of the variation in the distances of the luminaries. Calling the Sun's mean distance 1000, it is 983 in January and 1017 in July. The Moon's mean distance being 1000, she is at the distance 1055 when in apogeo, and 945 when in perigeo. The action of the luminaries in producing a change of figure varies in the inverse triplicate ratio of the distances (280.) Therefore, if 2 and 5 are taken for the mean disturbing forces of the Sun and Moon, we have the following measures of those forces:

	Sun.	*Moon.*
Apogean	1,901	4,258
Mean	2,—	5,—
Perigean	2,105	5,925

Hence we see that M : S may vary from 5,925 : 1,901 to 4,258 : 2,105, that is, nearly from 6 : 2 to 4 : 2.

The general expression of the disturbing force of the Moon will be $M = \frac{5}{2} S \times \frac{D^3}{\Delta^3} \times \frac{d^3}{\delta^3}$ where D and d express the mean distances of the Sun and Moon, and Δ and δ any other simultaneous distances.

The solar force does not greatly vary, and need not be much attended to in our computations for the tides. But the change in the lunar action must not be neglected, as this greatly affects both the time and the height of the tide.

418. First, as to the times.

1. The tide-day following spring-tide is 24ʰ 27½′ when the Moon is in perigeo, and 24ʰ 33′ when she is in apogeo.

2. The tide-day following neap-tide is 25ʰ 15′ in the first case, and 25ʰ 40′ in the second.

3. The greatest interval between the Moon's southing and high water (which happens in the octants) is 39′ when the Moon is in perigeo, and 61′ when she is in apogeo, y being 9° 45′ and 15° 15′.

419. The height of the tide is still more affected by the Moon's change of distance.

If the Moon is in perigeo, when new or full, the spring-tide will be eight feet, instead of the mean spring-tide of seven feet. The very next spring-tide will be no more than six feet, because the Moon is then in apogeo. The neap-tides, which happen between these very unequal tides, will be regular, the Moon being then in quadrature, at her mean distance.

But if the Moon change at her mean distance, the spring-tide will be regular, but one neap-tide will be four feet, and another only two feet.

We see therefore that the regular monthly series of heights and times corresponding to our construction can never be observed, because in the very same, or nearly the same period, the Moon makes all the changes of distance which produce the effects above mentioned. As the effect produced by the same change of the Moon's distance is different according to the state of the tide which it affects, it is by no means easy to apply the equation arising from this cause.

420. As a sort of synopsis of the whole of this description of the monthly series of tides, the following Table by Bernoulli will be of some use. The first column contains the Moon's elongation S M (eastward) from the Sun, or from the point opposite to the Sun, in degrees. The second column contains the minutes of solar time that the moment of high water precedes or follows the Moon's southing. This corresponds to the arch H M. The third column gives the arch S H, or nearly the hour and minute of the day at the time of high water; and the fourth column contains the height of the tide, as expresssed by the line *m a*, the space S *a* being divided into 1000 parts, as the height of a spring-tide. Note that the elongation is supposed to be that of the Moon at the time of her southing.

TABLE I.

S M	H M		Hour.	*m a*
	Minutes.			
0	—	Before the Moon's Southing.	-.—	1000
10	11		-.28½	987
20	22		-.58	949
30	31½		1.28½	887
40	40		2.—	806
50	45		2.35	715
60	46		3.13½	610
70	40½		3.59½	518
80	25		4.55	453
90	—		6.—	429
100	After the Moon's Southing.	25	7. 5	453
110		40½	8. ½	518
120		46½	8.46½	610
130		45	9.25	715
140		40	10.—	806
150		31½	10.31	887
160		22	11.2	949
170		11½	11.31	987
180		—	12.—	1000

420. It is proper here to notice a circumstance, of very general observation, and which appears inconsistent with our construction, which states the high water of neap-tides to happen when the Moon is on the meridian. This must make the high water of neap-tides six hours later than the high water of spring-tides, supposing that to happen when the Sun and Moon are on the meridian. But it is universally observed, that the high water of tides in quadrature is only about five hours and ten or twelve minutes later than that of the tides in syzigy.

This is owing to our not attending to another circumstance, namely, that the high water which happens in syzigy, and in quadrature, is not the high water of spring and of neap-tides, but the third before them. They correspond to a position of the Moon 19° westward of the syzigy or quadrature, as will be more particularly noticed afterwards. At these times, the points of high water are 13½ west of the syzigy, and 29 west of the quadrature, as appears by our construction. The lunar hours corresponding to the interval are exactly $5^h\ 02'$, which is nearly $5^h\ 12^m$ solar hours.

421. Hitherto we have considered the phenomena of the tides in their most simple state, by stating the Moon and the Sun in the equator. Yet this can never happen; that is, we can never see a monthly series of tides nearly corresponding with this situation of the luminaries. In the course of one month, the Sun may continue within six degrees of the equator, but the Moon will deviate from it, from 18 to 28 or 30 degrees. This will greatly affect the height of the tides, causing them to deviate from the series expressed by our construction. It still more affects the time, particularly of low water. The phenomena depend primarily on the zenith distances of the luminaries, and, when these are known, are accurately expressed by the construction. But these zenith distances depend both on the place of the luminaries in the heavens, and on the latitude of the observer. It is difficult to point out the train of phenomena as they occur in any one place, because the figure assumed by the waters, although its depth be easily ascertained in any single point, and for any one moment, is too complicated to be explained by any general description. It is not an oblong elliptical spheroid, formed by revolution, except in the very moment of new or full Moon. In other relative situations of the Sun and Moon, the ocean will not have any section that is circular. Its poles, and the position of its equator, are easily determined. But this equatorial section is not a circle, but approaches to an elliptical form, and, in some cases, is an exact ellipse. The longer axis of this oval is in the plane passing through the Sun and Moon, and its extremities

are in the points of low water for this circle, as determined by our construction. Its shorter axis passes through the centre of the Earth, at right angles to the other, and its extremities are the points of the *lowest low water*. In these two points, the depression below the natural level of the ocean is always the same, namely, the sum of the greatest depression produced by each luminary. It is subjected, therefore, only to the changes arising from the changes of distance of the Sun and Moon.

Thus it appears, that the surface of the ocean has generally four poles, two of which are prolate or protuberant, and two of them are compressed. This is most remarkably the case when the Moon is in quadrature, and there is then a ridge all round that section which has the Sun and Moon in its plane. The section through the four poles, upper and lower, is the place of high water all over the Earth, and the section perpendicular to the axis of this is the place of low water in all parts of the Earth.

Hence it follows, that when the luminaries are in the plane of the Earth's equator, the two depressed poles of the watery spheroid coincide with the poles of the Earth; and what we have said of the times of high and low water, and the other states of the tide, are exact in their application. But the heights of the tides are diminished as we recede from the Earth's equator, in the proportion of radius to the cosine of the latitude. In all other situations of the Sun and Moon, the phenomena vary exceedingly, and cannot easily be shewn in a regular train. The position of the high water section is often much inclined to the terrestrial meridians, so that the interval between the transit of the Moon and the transit of this section across the meridian of places in the same meridian, is often very different. Thus, on midsummer day, suppose the Moon in her last quadrature, and in the node, therefore in the equator. The ridge which forms high water lies so oblique to the meridians, that when the Moon arrives at the meridian of London, the ridge of high water has passed London about two hours, and is now on the north coast of America. Hence it happens, that we have no satisfactory account of the times of high water in different places, even though we should learn it for a particular day. The only way of forming a good guess of the state of the tides is to have a terrestrial globe before us, and having marked the places of the luminaries, to lap a tape round the globe, passing through these points, and then to mark the place of high water on that line, and cross it with an arch at right angles. This is the line of high water. Or, a circular hoop may be made, crossed by one semicircle. Place the circle so as to pass through the places of the Sun and Moon, setting the intersection with the semicircle on the calculated place of high water. The semicircle is now the line of high water, and if this armilla be held in its present position, while the globe turns once round within it; the succession of tide, or the regular hour of high water for every part of the Earth will then be seen, not very distant from the truth.

At present, in our endeavour to point out the chief modifications of the tides which proceed from the declination of the luminaries, or the latitude of the place of observation, we must content ourselves with an approximation, which shall not be very far from the truth. It will be sufficiently exact, if we attend only to the Moon. The effects of declination are not much affected by the Sun, because the difference between the declination of the Moon and that of the pole of the ocean can never exceed six or seven degrees. When the great circle passing through the Sun and Moon is much inclined to the equator (it may even be perpendicular to it), the luminaries are very near each other, and the Moon's place hardly deviates from the line of high water. At present, we shall consider the lunar tide only.

422. Let N Q S E (Fig. 50.) represent the terraqueous globe, N S being the axis, E Q the equator, and O the cen-

tre. Let the Moon be in the direction O M, having the declination B Q. Let D be any point on the surface of the Earth, and C D L its parallel of latitude, and N D S its meridian. Let B′ F b′ f be the elliptical surface of the ocean, having its poles B′ and b′ in the line O M. Let f O F be its equator.

As the point D is carried along the parallel C D L, it will pass in succession through all the states of the tide, having high water when it is in C, and in L, and low water when it gets into the intersection *d* of its parallel C L with the equator f *d* F of the watery spheroid. Draw the meridian N *d* G through this intersection, cutting the terrestrial equator in G. Then the arch Q G, converted into lunar hours, will give the duration of ebb of the superior tide, and G E is the time of the subsequent flood of the inferior tide. It is evident that these are unequal, and that the whole tide G Q G, consisting of a flood-tide G Q and ebb-tide Q G, while the Moon is above the horizon (which we called the *superior tide*), exceeds the duration of the whole *inferior tide* G E G by four times G O (reckoned in lunar hours.)

If the spheroid be supposed to touch the sphere in f and F, then C c′ is the height of the tide. At L, the height of the tide is L L′, and if the concentric circle L′ *q* be described, C′ *q* is the difference between the superior and inferior tides.

From this construction we learn, in general, that when the Moon has no declination, the duration of the superior and inferior tides of one day are equal over all the Earth.

423. 2. If the Moon has declination, the superior tide will be of longer or of shorter duration than the inferior tide, according as the Moon's declination B Q, and the latitude C Q of the place of observation are of the same or of different denominations.

424. 3. When the Moon's declination is equal to the colatitude of the place of observation, or exceeds it, that

is, if B Q is equal to N *o*, or exceeds it, there will be only a superior or inferior tide in the course of a lunar day. For, in this case, the parallel of the place of observation will pass through *f*, or between N and *f*, as *k m*.

425. 4. The sine of the arch G O is = tan. lat. × tan. declin. For rad. : cot. *d* O G = tan. *d* G : sin. G O, and sin. G O = tan. *d* G × cot. *d* O G. Now, *d* G is the latitude, and *d* O G is the codeclination.

426. The heights of the tides are affected in the same way by the declination of the Moon, and by the latitude of the place of observation. The height of the superior tide exceeds that of the inferior, if the Moon's declination is of the same denomination with the latitude of the place, and *vice versâ*. It often happens that the reverse of this is uniformly observed. Thus, at the Nore, in the entry to the river Thames, the inferior tide is greater than the superior, when the Moon has north declination, and *vice versâ*. But this happens because the tide at the Nore is only the derivation of the great tide which comes round the north of Scotland, ranges along the eastern coasts of Britain, and the high water of a superior tide arrives at the Nore, while that of an inferior tide is formed at the Orkney Islands, the Moon being under the horizon.

427. The height of the tide in any place, occasioned by the action of a single luminary, is as the square of the cosine of the zenith or nadir distance of that luminary. Hence we derive the following construction, which will express all the modifications of the lunar tide produced by declination or latitude. It will not be far from the truth, even for the compound tide, and it is perfectly exact in the case of spring or neap-tides.

With a radius C Q (Fig. 51.) taken as the measure of the whole elevation of a lunar tide, describe the circle E P Q *p*, to represent a terrestrial meridian, where P and *p* are the poles, and E Q the equator. Bisect C P in O, and round O describe the circle P B C D. Let M be that point of the meridian which has the Moon in the zenith, and let Z be the place of observation. Draw the diameter Z C N, cutting the small circle in B, and M C *m* cutting it in A. Draw A I parallel to E Q. Draw the diameter B O D of the inner circle, and draw I K, G H, and A F perpendicular to B D. Lastly, draw I D, I B, A D, A B, and C I M′, cutting the meridian in M′.

After half a diurnal revolution, the Moon comes into the meridian at M′, and the angle M′ C N is her distance from the nadir of the observer. The angle I C B is the supplement of I C N, and is also the supplement of I D B, the opposite angle of a quadrilateral in a circle. Therefore I D B is equal to the Moon's nadir distance. Also A D B, being equal to A C B, is equal to the Moon's zenith distance. Therefore, accounting D B as the radius of the tables, D F and D K are as the squares of the cosines of the Moon's zenith and nadir distances; and since P C, or D B, was taken as the measure of the whole lunar tide, D F will be the elevation of high water at the situation Z of the observer, when the Moon is above his horizon, and D K is the height of the subsequent tide, when the Moon is under his horizon, or, more accurately, it is the height of the tide seen at the same moment with D F, by a spectator at *z*′ in the same meridian and parallel. (For the *subsequent* tide, though only twelve hours after, will be a little greater or less, according as they are on the increase or decrease.) D F, then, and D K, are proportional to the heights of the superior and inferior tides of that day. Moreover, as A I is bisected in G, F K is bisected in H, and D H is the arithmetical mean between the heights of the superior and inferior tides. Accounting O C as the radius of the tables, A G is the sine of the arch A C, which measures twice the angle M C Q, the Moon's declination. O G is the cosine of twice the Moon's declination. Also the angle B O G is equal to twice the angle B C Q, the latitude of the observer. Therefore O H = cos. 2 decl.

$\times \cos.\ 2\ \text{lat.}$; and $DH = DO + OH$, $= M \times \frac{1 + \cos.\ 2\ \text{decl.}\ \text{☾} \times \cos.\ 2\ \text{lat.}}{2}$. This value of the medium tide will be found of continual use.

This construction gives us very distinct conceptions of all the modifications of the height of a lunar tide, proceeding from the various declinations of the Moon, and the position of the observer; and the height of the compound tide may be had by repeating the construction for the Sun, substituting the declination of the Sun for that of the Moon, and S for M in the last formula. The two elevations being added together, and $\frac{1}{2}\ \overline{M + S}$ taken from the sum, we have the height required. If it is a spring-tide that we calculate for, there is scarcely any occasion for two operations, because the Sun cannot then be more than six degrees from the Moon, and the pole of the spheroid will almost coincide with the Moon's place. We may now draw some inferences from this representation.

428. 1. The greatest tides happen when the Moon is in the zenith or nadir of the place of observation. For as M approaches to Z, A and I approach to B and D, and when they coincide, F coincides with B, and the height of the superior tide is then $= M$. The medium tide, however, diminishes by this change, because G comes nearer to O, and consequently H comes also nearer to O, and D H is diminished.

If, on the other hand, the place of observation be changed, Z approaching to M, the superior, inferior, and medium tides are all increased. For, in such case, D separates from I, and DK, DH, and DF are all enlarged.

429. 2. If the Moon be in the equator, the superior and inferior tides are equal, and $= M \times \cos.^2\ \text{lat.}$ For then A and I coincide with C; and F and K coalesce in i; and $D i = DB \times \cos.^2\ BDC, = DB \times \cos.^2\ ZCQ$.

430. 3. If the place of observation be in the equator, the superior and inferior tides are equal every where, and

are $= M \times \cos.^2$, declin. ☾. For B then coincides with C; the points F and K coincide with G; and $PG = PC \times \cos.^2 CPA, = M \times \cos.^2 MCQ$.

431. 4. The superior tides are greater or less than the inferior tides, according as Z and M are on the same or on opposite sides of the equator. For, by taking QZ' on the other side of the equator, equal to QZ, and drawing $Z'Cz'$, cutting the small circle in β, we see that the figure is simply reversed. The magnitudes and proportions of the tides are the same in either case, but the combination is inverted, and what belongs to a superior tide in the one case belongs to an inferior tide in the other.

432. 5. If the colatitude be equal to the Moon's declination, or less than it, there will be no inferior tide, or no superior tide, according as the latitude and Moon's declination are of the same or of different denominations. For when $PZ = MQ$, D coincides with I, and K also coincides with I. Also, when PZ is less than MQ, D falls below I, and the point Z never passes through the equator of the watery spheroid. The low water $m\,m$ (Fig. 50.) observed in the parallel $k\,m$ is only a lower part of the same tide $k\,k'$, of which the high water is also observed in the same place. In such situations, the tides are very small, and are subjected to singular varieties, which arise from the Moon's change of declination and distance. Such tides can be seen only in the circumpolar regions. The inhabitants of Iceland notice a period of nineteen years, in which their tides gradually increase and diminish, and exhibit very singular phenomena. This is undoubtedly owing to the revolution of the Moon's nodes, by which her declination is considerably affected. That island is precisely in the part of the ocean where the effect of this is most remarkable. A register kept there would be very instructive; and it is to be hoped that this will be done, as in that sequestrated Thulé there is a zealous astronomer, M. Lievog, furnished with good instruments, to whom this series of observations has been recommended.

433. 6. At the very pole there is no daily tide. But there is a gradual rise and subsidence of the water twice in a month, by the Moon's declining on both sides of the equator. The water is lowest at the pole when the Moon is in the equator, and it rises about twenty-six inches when the Moon is in the tropics. Also, when her ascending node is in the vernal equinox, and she has her greatest declination, the water will be thirty inches above its lowest state, by the action of the Moon alone.

434. 7. The medium tide is, as has already been observed, $= M \times \frac{1 + \cos. 2 \text{ decl.} ☾ \times \cos. 2 \text{ lat.}}{2}$.

As the Moon's declination never exceeds 30°, the cosine of twice her declination is always a positive quantity, and never less than $\frac{1}{2}$. When the latitude is less than 45°, the cosine of twice the latitude is also positive, but negative when the latitude exceeds 45°. Attending to these circumstances, we may infer,

435. 1. That the mean tides are equally affected by the northerly and southerly declinations of the Moon.

436. 2. If the latitude be exactly 45°, the mean tide is always the same, and $= \frac{1}{2}$ M. For, in this case, B D is perpendicular to P C, and the point H always coincides with O. This is the reason why, on the coasts of France and Spain, the tides are so little affected by the declination of the luminaries.

437. 3. When the latitude is less than 45°, the mean tides increase as the declination of the Moon diminishes. For *cosin. 2 lat.* being then a positive quantity, the formula increases when the cosine of the declination of the Moon increases, that is, it diminishes when the declination of the Moon increases. As B Q diminishes, G comes nearer to C, and H separates from O towards B, and D H increases.

But if the latitude exceed 45°, the point H must fall between O and D, and the mean tide will increase as the declination increases.

498. 5. If the latitude be = 0, the point H coincides with G, and the effect of the Moon's declination is then the most sensible. The mean tide, in this case, is M × $\frac{1 + \cos. 2 \text{ declin. ☾}}{2}$.

499. Every thing that has been determined here for the lunar tide may easily be accommodated to the high and low water of the compound tide, by repeating the computations with S in the place of M, as the constant coefficient. But, in general, it is almost as exact as the nature of the question will admit, to attend only to the lunar tide. The declination of the real summit of the spheroid, in this case, never differs from the declination of the summit of the lunar tide more than two degrees, and the correction may be made at any time by a little reflection on the simultaneous position of the Sun. What has been said is strictly applicable to the spring tides.

$\overline{M + S - \text{tide}} \times \sin.^2 d\,O$ (Fig. 50.) is the quantity to be added to the tide found by the construction. It is exact in spring-tides, and very near the truth in all other cases. The $\sin.^2 d\,O$ is $= \frac{8^2 \text{ lat.}}{\cos.^2 \text{decl. ☾}}$. For $\sin. d\,O\,G : \sin. d\,G\,O = \sin. d\,G : \sin. d\,O$.

Such, then, are the more simple and general consequences of gravitation on the waters of our ocean, on the supposition that the whole globe is covered with water, and that the ocean always has the form which produces a perfect equilibrium of force in every particle.

404. But the globe is not so covered, and it is clear that there must be a very great extent of open sea, in order to produce that elevation at the summit of the spheroid which corresponds with the accumulating force of the luminaries. A quadrant at least of the ellipse is necessary for giving the whole tide. With less than this, there will not be enough of water to make up the spheroid. And, to produce the full daily vicissitude of high and low water, this extent of

sea must be in longitude. An equal extent in latitude may produce the greatest elevation; but it will not produce the series of heights that should occur in the course of a lunar day. In confined seas of small extent, such as the Caspian, the Euxine, the Baltic, and the great lakes in North America, the tides must be almost insensible. For it is evident that the greatest difference of height on the shore of such confined seas can be no more than the deflection from the tangent of the arch of the spheroid contained in that sea. This, in the Caspian Sea, cannot exceed seven inches; a quantity so small, that a slight breeze of wind, setting off shore, will be sufficient for preventing the accumulation, and even for producing a depression. A moderate breeze, blowing along the canal in St James's Park at London, raises the water two inches at one end, while it depresses it as much at the other. The only confined seas of considerable extent are the Mediterranean and the Red Sea. The first has an extent of 40° in longitude, and the tides there might be very sensible, were it on the equator, but being in lat. 35 nearly, the effects are lessened in the proportion of five to four. In such a situation, the phenomena are very different, both in regard to time and to kind, from what they would be, if the Mediterranean were part of the open ocean. Its surface will be *parallel* to what it would be in that case, but *not the same*. This will appear by inspection of Fig. 51, where *m r p* represents the natural level of the ocean, and M *o* Q represents the watery spheroid, having its pole in M, and its equator at Q. S *s* may represent a tide post, set up on the shore of Syria, at the east end of the Mediterranean, and G *o* a post set up at the Gut of Gibraltar, which we shall suppose at present to be dammed up. When the Moon is over M, the waters of the Mediterranean assume the surface *g r s*, parallel to the corresponding portion of the elliptical surface Q *o* M, crossing the natural surface at *r*, nearly in the middle of its length. Thus, on the Syrian coast, there is a considerable elevation of the waters, and at Gibraltar, there is a considerable depression. In the middle of the length, the water is at its mean height. The water of the Atlantic Ocean, an open and extensive sea, assumes the surface of the equilibrated spheroid, and it stands considerably higher on the outside of the dam, as is seen by G *o*, than on the inside, as expressed by G *g*. It is nearly low water within the Straits, while it is about $\frac{1}{3}$ or $\frac{1}{2}$ flood without. The water has been ebbing for some hours within the Straits, but flowing for great part of the time without. As the Moon moves westward, toward Gibraltar, the water will begin to rise, but slowly, within the Straits, but it is flowing very fast without. When the Moon gets to P, things are reversed. The summit of the spheroid (it being supposed a spring-tide) is at P, and it is nearly high water within the Straits, but has been ebbing for some hours without. It is low water on the coast of Syria. All this while, the water at *r*, in the middle of the Mediterranean, has not altered its height by any sensible quantity. It will be high water at one end of the Mediterranean, and low water at the other, when the middle is in that part of the general spheroid where the surface makes the most unequal angles with the vertical. This will be nearly in the octants, and therefore about $1\frac{3}{4}$ hours before and after the Moon's southing (supposing it spring-tide).

These observations greatly contribute to the explanation of the singular currents in the Straits of Gibraltar, as they are described by different authors. For although the Mediterranean is not shut up, and altogether separated from the Atlantic Ocean at Gibraltar, the communication is extremely scanty, and by no means sufficient for allowing the tide of the ocean to diffuse itself into this bason in a regular manner. Changes of tide, always different, and frequently quite opposite, are observed on the east and west side of the narrow neck which connects the Rock with Spain; and the general tenor of those changes has a very great analogy with what has now been described. The tides in the Mediterranean are small, and therefore easily affected by winds. But they are remarkably regular. This may be expected. For as the collection or abstraction necessary for producing the change is but small, they are soon accomplished. The registers of the tides at Venice and some other ports in the Adriatic are surprisingly conformable to the theory. See Phil. Trans. vol. LXVII.

From this example, it is evident that great deviations may be expected in the observed phenomena of the tides from the immediate results of the simple unobstructed theory, and yet the theory may be fully adequate to the explanation of them, when the circumstances of local situation are properly considered.

405. The real state of things is such, that there are very few parts of the ocean, where the theory can be applied without very great modifications. Perhaps the great Pacific Ocean is the only part of the terraqueous globe in which all the forces have room to operate. When we consider the terrestrial globe as placed before the acting luminaries, which have a relative motion round it from east to west, and consider the accumulation of the waters as keeping pace with them on the ocean, we must see that the tides with which we are most familiarly acquainted, namely, those which visit the western shores of Europe and Africa, and the eastern shores of America, must also be irregular, and be greatly diversified by the situation of the coasts. The accumulation on our coasts must be in a great measure supplied by what comes from the Indian and Ethiopic Ocean from the eastward, and what is brought, or kept back, from the South Sea; and the accumulation must be diffused, as from a collection coming round the Cape of Good Hope, and round Cape Horn. Accordingly, the propagation of high water is entirely consonant with such a supposition. It is high water at the Cape of Good Hope about

three o'clock at new and full moon, and it happens later and later, as we proceed to the northward along the coast of Africa; later and later still as we follow it along the west coasts of Spain and France, till we get to the mouth of the English Channel. In short, the high water proceeds along those shores just like the top of a wave, and it may be followed, hour after hour, to the different harbours along the coast. The same wave continues its progress northwards (for it seems to be the only supply), part of it going up St George's Channel, part going northward by the west side of Ireland, and a branch of it going up the English Channel, between this island and France. What goes up by the east and west sides of Ireland unites, and proceeds still northward, along the western coasts and islands of Scotland, and then diffuses itself to the eastward, toward Norway and Denmark, and, circling round the eastern coasts of Britain, comes southward, in what is called the German Ocean, till it reaches Dover, where it meets with the branch which went up the English Channel.

406. It is remarkable that this northern tide, after having made such a circuit, is more powerful than the branch which proceeds up the English Channel. It reaches Dover about a quarter of an hour before the southern tide, and forces it backwards for half an hour. It must also be remarked, that the tide which comes up channel is not the same with the tide which meets it from the north, but is a whole tide earlier, if not two tides. For the spring-tide at Rye is a tide earlier than the spring-tide at the Nore. It even seems more nearly two tides earlier, appearing the one as often as the other. This may be better seen by tracing the hour of high water from the Lizard up St George's Channel and along the west coasts of Scotland. Now it is very clear that the superior tide at the Orkney islands is simultaneous with the inferior tide at the mouth of the Thames. It is therefore most probable that the Orkney tide is at least one tide later than at the Lizard. The whole

of this tide is very anomalous, especially after getting to the Orkneys. It is a derivative from the great tide of the open sea, which being very distant, is subjected to the influence of hard gales, at a distance, and frequently unlike what is going on upon our coasts.

407. A similar progress of the same high water from the southward is observed along the eastern shores of South America. But, after passing Brazil and Surinam, the Atlantic Ocean becomes so wide that the effect of this high water, as an adventitious thing supplied from the southward, is not so sensible, because the Atlantic itself is now extensive enough to contribute greatly to the formation of the regular spheroid. But it contributes chiefly by abstraction of the waters from the American side, while the accumulation is forming on the European side of the Atlantic. By studying the successive hours of high water along the western coasts of Africa and Europe, it appears that it takes nearly two days, or between four and five tides, to come from the Cape of Good Hope to the mouth of the English Channel. This remark is of peculiar importance.

408. Few observations have, as yet, been made public concerning the tides in the Great Pacific Ocean. They must exhibit phenomena considerably different from what are seen in the Atlantic. The vast stretch of uninterrupted coast from Cape Horn to Cook's Straits, prevents all supply from the eastward for making up the spheroid. So far as we have information, it appears that the tides are very unlike the European tides, till we get 40° or 50° west from the coast of America. In the neighbourhood of that coast, there is scarcely any inferior tide. Even in the middle of the vast Pacific Ocean the tides are very small, but abundantly regular.

409. The setting of the tides is affected, not only by the form of the shores, but also by the inequalities which undoubtedly obtain in the bottom of the ocean. A deep and long valley there will give a direction to the waters which

move along it, even although they far overtop the higher parts on each side, just as we observe the wind follow the course of the valleys. This direction of the undermost waters affects those that flow above them, in consequence of the mutual adhesion of the filaments; and thus the whole stream is deflected from the direction which it would have taken, had the ground been even. By such deflections the path is lengthened, and the time of its reaching a certain place is protracted; and this produces other deviations from the calculations by the simple theory.

446. These peculiarities in the bed or channel also greatly affect the height of the tides. When a wave of a certain magnitude enters a channel, it has a certain quantity of motion, measured by the quantity of water and its velocity. If the channel, keeping the same depth, contract in its width, the water, keeping for a while its momentum, must increase its velocity, or its depth, or both. And thus it may happen that, although the greatest elevation produced by the joint action of the Sun and Moon in the open sea does not exceed eight or nine feet, the tide in some singular situations may mount considerably higher. It seems to be owing to this that the high water of the Atlantic Ocean, which at St Helena does not exceed four or five feet, setting in obliquely on the coast of North America, ranges along that coast, in a channel gradually narrowing, till it is stopped in the Bay of Fundy as in a hook, and there it heaps up to an astonishing degree. It sometimes rises 120 feet in the harbour of Anapolis-Royal. Were it not that we see instances of as strange effects of a sudden check given to the motion of water, we should be disposed to think that the theory is not adequate to the explanation of the phenomena. But the extreme disparity that we may observe in places very near each other, and which derive their tide from the very same tide in the open sea, must convince us that such anomalies do not impugn the general principle,

although we should never be able fully to account for the discrepancy.

447. Nothing causes so much irregularity in the tides as the reflection of the tide from shore to shore. If a pendulum, while vibrating, receives little impulses, at intervals that are always the same, and very nearly equal to its own vibrations, or even to an aliquot part of them, the vibrations may be increased to a great magnitude after some time, and then will gradually diminish, and thus have periods of increase and decrease. So it happens in the undulation which constitutes a tide. The situation of the coasts may be such, that the time in which this undulation would, of itself, play backward and forward from shore to shore, may be so exactly fitted to the recurring action of the Moon, that the succeeding impulses, always added to the natural undulation, may raise it to a height altogether disproportioned to what the action of the Moon can produce in open sea, where the undulation diffuses itself to a vast distance. What we see in this way should suffice for accounting for the great height of the tides on the coasts of continents. Dan. Bernoulli, justly thinking that the obstructions of various kinds to the movements of the ocean should make the tides less than what the unobstructed forces are able to produce, concluded, from the great tides actually observed, and compared with the tides producible by the Newtonian theory, that this theory was erroneous. He thought it all derived from Newton's erroneous idea of the proportion of the two axes of the terraqueous globe; which mistake results from the supposition of primitive fluidity and uniform density. He investigates the form of the Earth, accommodated to a nucleus of great density, covered with a rarer fluid, and he thinks that he has demonstrated that the height of the tide will be in proportion to the comparative density of this nucleus, or the rarity of the fluid. This, says he, alone can account for the tides that we really observe; and which, great as they are, are

certainly only a part of what they would be, were they not so much obstructed. This is extremely specious, and, coming from an eminent mathematician, has considerable authority. But the problem of the figure of the Earth has been examined with the most scrupulous attention, since the days of M. Bernoulli, by the first mathematicians of Europe, who are all perfectly agreed in their deductions, and confirm that of Sir Isaac Newton. They have also proved, and we apprehend that it is sufficiently established in art. 420. that a denser nucleus, instead of making a greater tide, will make it smaller than if the whole globe be of one density. The ground of Bernoulli's mistake has also been clearly pointed out. There remains no other way of accounting for the great tides but by causes such as have now been mentioned. When the tides in the open Pacific Ocean never exceed three or four feet, we must be convinced that the extravagant tides observed on the coasts of great continents are anomalies; for there, the obstructions are certainly greater than in the open sea. We must therefore look for an explanation in the motions and collisions of disturbed tides. These anomalies therefore bring no valid objection against the general theory.

448. There are some situations where it is easy to explain the deviations, and the explanation is instructive. Suppose a great navigable river, running nearly in a meridional direction, and falling into the sea in a southern coast. The high water of the ocean reaches the mouth of this river (we may suppose) when the Sun and Moon are together in the meridian. It is therefore a spring-tide high water at the mouth of the river at noon. This checks the stream at the mouth of the river, and causes it to deepen. This again checks the current farther up the river, and it deepens there also, because there is always the same quantity of land water pouring into it. The stream is not perhaps stopped, but only retarded. But this cannot happen without its growing deeper. This is propagated farther

and farther up the stream, and it is perceived at a great distance up the river. But this requires a considerable time. Our knowledge in hydraulics is too imperfect as yet to enable us to say in what number of hours this sensible check, indicated by the smaller velocity, and greater depth, will be propagated to a certain distance. We may suppose it just a lunar day before it arrive at a certain wharf up the river. The Moon, at the end of the day, is again on the meridian, as it was when it was a spring-tide at the mouth of the river the day before. But, in this interval, there has been another high water at the mouth of the river, at the preceding midnight, and there has just been a third high water, about fifteen minutes before the Moon came to the meridian, and thirty-five minutes after the Sun has passed it. There must have been two low waters in the interval, at the mouth of the river. Now, in the same way that the tide of yesterday noon is propagated up the stream, the tide of midnight has also proceeded upwards. And thus, there are three coexistent high waters in the river. One of them is a spring-tide, and it is far up, at the wharf above mentioned. The second, or the midnight tide, must be half way up the river, and the third is at the mouth of the river. And there must be two low waters intervening. The low water, that is, a state of the river below its natural level, is produced by the passing low water of the ocean, in the same way that the high water was. For when the ocean falls below its natural level at the mouth of the river, it occasions a greater declivity of the issuing stream of the river. This must augment its velocity—this abstracts more water from the stream above, and that part also sinks below its natural level, and gives a greater declivity to the waters behind it, &c. And thus the stream is accelerated, and the depth is lessened, in succession, in the same way as the opposite effects were produced. We have a low water at different wharfs in succession, just as we had the high waters.

449. This state of things, which must be familiarly known to all who have paid any attention to these matters, being seen in almost every river which opens into a tide way, gives us the most distinct notion of the mechanism of the tides. The daily returning tide is nothing but an undulation or wave, excited and maintained by the action of the Sun and Moon. It is a great mistake to imagine that we cannot have high water at London Bridge (for example) unless the water be raised to that level all the way from the mouth of the Thames. In many places that are far from the sea, the stream, at the moment of high water, is down the river, and sometimes it is considerable. At Quebec, it runs downward at least three miles per hour. Therefore the water is not heaped up to the level; for there is no stream without a declivity. The harbour at Alloa, in the river Forth, is dry at low water, and the bottom is about six feet higher than the highest water-mark on the stone pier at Leith. Yet there are at Alloa tides of twenty, and even twenty-two feet. All Leith would then be under water, if it stood level from Alloa at the time of high water there.

After considering a tide in this way, any person who has remarked the very strange motions of a tide river, in its various bendings and creeks, and the currents that are frequently observed in a direction opposite to the general stream, will no longer expect that the phenomena of the tides will be such as immediately result from the regular operation of the solar and lunar forces.

450. There is yet another cause of deviation, which is perhaps more dissimilating than any local circumstances, and the operation of which it is very difficult to state familiarly, and yet precisely. This is the inertia, as it is called, of the waters. No finite change of place or of velocity can be produced in an instant by any accelerating force. Time must elapse before a stone can acquire any measureable velocity by falling.

Suppose the Earth fluid to the centre, and at rest, without any external disturbing force. The ocean will form a perfect sphere. Let the Moon now act on it. The waters will gradually rise immediately under the Moon and in the opposite part of the Earth, sinking all around the equator of the spheroid. Each particle proceeds to its ultimate situation with an accelerated motion, because, till then, the disturbing force exceeds the tendency of the water to subside. Therefore, when the form is attained which balances those forces, the motion does not stop, just as a pendulum does not stop when it reaches the lowest point of its arch of vibration. Suppose that the Moon ceases to act at this instant. The motion will still go on, and the ocean will overpass the balanced figure, but with a retarded motion, as the pendulum rises on the other side of the perpendicular. It will stop at a certain form, when all the former acceleration is done away by the tendency of the water to subside. It now begins to subside at the poles of the spheroid, and to rise at the equator, and after a certain time, it becomes a perfect sphere, that is, the ocean has its natural figure. But it passes this figure as far on the other side, and makes a flood where there was formerly an ebb; and it would now oscillate for ever, alternately swelling and contracting at the points of syzigy and quadrature. If the Moon do not cease to act, as was just now supposed, there will still be oscillations, but somewhat different from those now mentioned. The middle form, on both sides of which it oscillates in this case, is not the perfect sphere, but the balanced spheroid.

451. All this is on the supposition that there is no obstruction. But the mutual adhesion of the filaments of water will greatly check all these motions. The figure will not be so soon formed; it will not be so far overpassed in the first oscillation; the second oscillation will be less than the first, the third will be less than the second, and they will soon become insensible.

But if it were possible to provide a recurring force, which should tend to raise the waters where they are already rising, and depress them where they are subsiding, and that would always renew those actions in the proper time, it is plain that this force may be such as will just balance the obstructions competent to any particular degree of oscillation. Such a recurring force would just maintain this degree of oscillation. Or the recurring force may be greater than this. It will therefore increase the oscillations, till the obstructions are also so much increased that the force is balanced by them. Or it may be less than what will balance the obstructions to the degree of oscillation excited. In this case the oscillation will decrease, till its obstructions are no more than what this force will balance. Or this recurring force may come at improper intervals, sometimes tending to raise the waters when they are subsiding in the course of an oscillation, and depressing them when they are rising. Such a force must check and greatly derange the oscillations; destroying them altogether, and creating new ones, which it will increase for some time, and then check and destroy them; and will do this again and again.

Now there is such a recurring force. As the Earth turns round its axis, suppose the form of the balanced spheroid attained in the place immediately under the Moon. This elevation or pole is carried to the eastward by the Earth, suppose into the position D O B (Fig. 58.), the Moon being in the line O M. The pole of the watery spheroid is no longer under the Moon. The Moon will therefore act on it so as to change its figure, making it subside in the remote quadrant B *b* C, and rise a little in the quadrant B *a* A. Thus its pole will come a little nearer to the line O M. It is plain, that if B is carried farther eastward, but within certain limits, the situation of the particles will be still more unsuitable to the lunar disturbing force, and its action on each to change its position will be greater. The action upon them all will therefore make a more rapid change in the position of the pole of the displaced spheroid. It seems not impossible that this pole may be just so far east—that the changing forces may be able to cause its pole to shift its position fifteen miles in one minute. If this be the case, the pole of the spheroid will keep precisely at its present distance from the line O M. For, since it would shift to the westward fifteen miles in one minute by the action of the Moon, and is carried fifteen miles to the eastward in that time by the rotation of the Earth, the one motion just undoes the effect of the other. The pole of the watery spheroid is really made to shift fifteen miles to the westward on the surface of the Earth, and arrives at a place fifteen miles west of its former place on the globe; but this place of arrival is carried fifteen miles to the eastward; it is therefore as far from the line O M as before.

This may be illustrated by a very simple experiment, where the operation of the acting forces is really very like that of the lunar disturbing force. Suppose a chain or flexible rope A B C E D F laid over a pulley, and hanging down in a bight, which is a catenarean curve, having the vertical line O D for its axis, and D for its lowest point, which the geometers call its vertex. Let the pulley be turned very slowly round its axis, in the direction A B C. The side C E will descend, and F A will be taken up, every link of the chain moving in the curve C E D F A. Every link is in the vertex D in its turn, just as every portion of the ocean is in the vertex or pole of the spheroid in its turn. Now let the pulley turn round very briskly. The chain will be observed to alter its figure and position. O D will no longer be its axis, nor D its vertex. It will now form a curve C *e d f* A, lying to the left hand of C E D F A. O*d* will be its new axis, and *d* will be its vertex. Gravity acts in lines parallel to O D. The motions in the direction C E and F A nearly balance each other. But there is a general motion of every link of the hanging chain, by which it is carried from E towards F. Did the chain continue in the former catenarea, this force could not be balanced. It therefore keeps so much awry, in the form C *e d f* A, that its tendency by gravity to return to its former position is just equal to the sum of all the motions in the links from E towards F. And it will shew this tendency by returning to that position, the moment that the pulley gives over turning. The more rapidly we turn the pulley round, the farther will the chain go aside before its attitude become permanent.

452. It surpasses our mathematical knowledge to say with precision how far eastward the pole of the tide must be from the line of the Moon's direction, even in the simple case which we have been considering. The real state of things is far more complicated. The Earth is not fluid to the centre, but is a solid nucleus, on which flows an ocean of very small depth. In the former case, a very moderate motion of each particle of water is sufficient for making the accumulation in one place, and the depression in another. The particles do little more than rise or subside vertically. But, in the case of a nucleus covered with an ocean of small depth, a considerable horizontal motion is required for bringing together the quantity of water wanted to make up the balanced spheroid. The obstructions to such motion must be great, both such as arise from the mutual adhesion of the filaments of water, and many that must arise from friction and the inequalities of the bottom, and the configuration of the shores. In some places, the force of the acting luminaries may be able to cause the pole of the spheroid to shift its situation as fast as the surface moves away, when the angle M O B is 20°. In other places, this may not be till it is 25°, and in another, 15° may be enough; but, in every situation, there will be an arrangement that will produce this permanent position of the summit. For when the obstructions are great, the balanced form will not be nearly attained; and when this is the case, the change pro-

ducible on the position of a particle is more rapidly effected, the forces being great, or rather the resistance arising from gravity alone being small.

453. The consequence of all this must be, in the first place, that that form which the ocean would ultimately assume, did the Earth not turn round its axis, will never be attained. As the waters approach to that form, they are carried eastward, into situations where the disturbing forces tend to depress them on one side, while they raise them on the other, causing a westerly undulation, which keeps its summit at nearly the same distance from the line of the acting luminary's direction. This westerly motion of the summit of the undulation does not necessarily suppose a real transference of the water to the westward at the same rate. It is more like the motion of ordinary waves, in which we see a bit of wood or other light body merely rise and fall without any sensible motion in the direction of the wave. In no case whatever is the horizontal motion of the water nearly equal to the motion of the summit of the wave. It resembles an ordinary wave also in this, that the rate at which the summit of the undulation advances in any direction is very little affected by the height of the wave. Our knowledge, however, in hydraulics has not yet enabled us to say with precision what is the relation between the height of the undulation and the rate of its advance.

454. Thus then it appears, in general, that the summit of the tide must always be to the eastward of the place assigned to it by our simple theory, and that experience alone can tell us how much. Experience is more uniform in this respect than one should expect. For it is a matter of almost universal experience that it is very nearly 19 or 29 degrees. In a few places it is less, and in many it is 5, or 6, or 7 degrees more. This is inferred from observing that the greatest and the smallest of all the tides do not happen on the very time of the syzigies and quadratures,

but the third, and in some places, the fourth tide after. Subsequent observation has shewn that this is not peculiar to the spring and neap-tides, but obtains in all. At Brest (for example) the tide which bears the mark of the augmentation arising from the Moon's proximity is not the tide seen while the Moon is in perigeo, but the third after. In short, the whole series of monthly tides disagree with the simultaneous position of the luminaries, but correspond most regularly with their positions 37 or 38 hours before.

455. Another observation proper for this place is, that as different extent of sea, and different depth of water, will and do occasion a difference in the time in which a great undulation may be propagated along it, it may happen that this time may so correspond with the repetition of all the agitating forces, that the action of to-day may so conspire with the remaining undulation of yesterday as to increase it by its reiterated impulses, to a degree vastly greater than its original quantity. By giving gentle impulses in this way to a pendulum, in the direction of its motion, its vibrations may be increased to fifty times their first size. It is not necessary, for this effect, that the return of the luminary into the favourable situation be just at the interval of the undulation. It will do if it conspire with every second or third or fourth undulation; or, in general, if the amount of its conspiring actions exceeds considerably, and at no great distance of time, the amount of its opposing actions. In many cases, this co-operation will produce periods of augmentation and diminution, and many seeming anomalies, which may greatly vary the phenomena.

456. A third observation that should be made here is, that as the obstructions to the motion of the ocean arising from the mutual adhesion and action of the filaments are known to be so very great, we have reason to believe that the change of form actually produced is but a moderate part of what the force can ultimately produce, and that none of the oscillations are often repeated. It is not pro-

bable that the repetitions of the great undulations can much exceed four or five. When experiments are made on still water, we rarely see a pure undulation repeated so often. Even in a syphon of glass, where all diffusions of the undulating power is prevented, they are rarely sensible after the fifth or sixth. A gentle smooth undulation on the surface of a very shallow bason, in the view of agitating the whole depth, will seldom be repeated thrice. This is the form which most resembles a tide.

457. After this account of the many causes of deviation from the motions assigned by our theory, many of which are local, and reducible to no rule, it would seem that this theory, which we have taken so much pains to establish, is of no use, except that of giving us a general and most powerful argument for the universal gravitation of matter. But this would be too hasty a conclusion. We shall find that a judicious consideration of the different classes of the phenomena of the tides will suggest such relations among them, that, by properly combining them, we shall not only perceive a very satisfactory agreement with the theory, but shall also be able to deduce some important practical inferences from it.

458. Each of the different modifications of a tide has its own period, and its peculiar magnitude. Where the change made by the acting force is but small, and the time in which it is effected is considerable, we may look for a considerable conformity with the theory: but, on the other hand, if the change to be produced on the tide is very great, and the time allowed to the forces for effecting it is small, it is equally reasonable to expect sensible deviations. If this consideration be judiciously applied, we shall find a very satisfactory conformity.

459. Of all the modifications of a tide, the greatest, and the most rapidly effected, is the difference between the superior and inferior tides of the same day. When the Moon has great declination, the superior tide at Brest may be

three times greater than the succeeding or inferior tide. But the fact is, that they differ very little. M. de la Place says that they do not differ at all. We cannot find out his authority. Having examined with the most scrupulous attention more than 200 of the observations at Brest, and Rochefort, and Port L'Orient, and made the proper allowance for the distances of the luminaries, we can say with confidence, that this general assertion of M. de La Place is not founded on the observations that have been published; and it does not agree with what is observed in the other ports of Europe. There is always observed a difference, agreeing with theory in the proportions, and in the order of their succession, although much smaller. A very slight consideration will give us the reason of the observed discrepancy. It is not possible to make two immediately succeeding undulations of inert water remarkably different from each other. The great undulation, in retiring, causes the water to heap up to a greater height in the offing; and this, in diffusing itself, must make the next undulation greater on the shore. That this is the true account of the matter, is fully proved by observing, that when the theoretic difference between those two tides is very small, it is as distinctly observed in the harbours as when it is great. This is clearly seen in the Brest observations.

460. The absolute magnitudes of the tides are greatly modified by local circumstances. In some harbours there is but a small difference between the spring and neap-tides, and in other harbours it is very great. But, in either case, the small daily changes are observed to follow the proportion required by the theory with abundant precision. Counted half way from the spring to the neap-tides, the hourly fall of the tide is as the square of the time from spring-tide, except so far as this may be changed by the position of the moon's perigee. In like manner, the hourly increase of the tides after neap-tide is observed to be as the squares of the time from neap-tide.

461. The priming and lagging of the tides corresponds with the theory with such accuracy, that they seem to be calculated from it, independent of observation. There is nothing that seems less likely to be deranged than this. Tides which differ very little from each other, either as to magnitude or time, should be expected to follow one another just as the forces require. There is indeed a deviation, very general, and easily accounted for. There is a small acceleration of the tides from spring-tide to neap-tide. This is undoubtedly owing to the obstructions. A smaller tide being less able to overcome them, is sooner brought to its maximum. The deviation, however, is very small, not exceeding ¼ of an hour, by which the neap-tide anticipates the theoretical time of its accomplishment. It would rather appear at first sight that a small tide would take a longer time of going up a river than a great one. And it may be so, although it be sooner high water, because the defalcation from its height may sooner terminate its rising. There is no difference observed in this respect, when we compare the times of high water at London Bridge and at the Buoy of the Nore. They happen at the very same time at both places, and therefore the spring-tides and the neap-tides employ the same time in going up the river Thames.

462. This agreement of observation with theory is most fortunate; and indeed without it, it would scarcely have been possible to make any practical use of the theory. But now, if we note the exact time of the high water of spring-tide for any harbour, and the exact position of the Sun and Moon at that time, we can easily make a table of the monthly series for that port, by noticing the difference of that time from our table, and making the same difference for every succeeding phasis of the tide.

463. But, in thus accommodating the theoretical series to any particular place, we must avoid a mistake commonly made by the composers of tide-tables. They give the

hour of high water at full and change of the Moon, and this is considered as spring-tide. But perhaps there is no part in the world where that is the case. It is usually the third tide after full or change that is the greatest of all; and the third tide after quadrature is, in most places, the smallest tide. Now it is with the greatest tide that our monthly series commences. Therefore, it is the hour of *this* tide that is to be taken for the hour of the harbour. But, as winds, freshes, and other causes, may affect any individual tide, we must take the medium of many observations; and we must take care that we do not consider as a spring-tide one which is indeed the greatest, but chances to be enlarged by being a perigean tide.

When these precautions are taken, and the tides of one monthly series marked, by applying the same correction to the hours in the third column of Bernoulli's table (I.), it will be found to correspond with observation with sufficient accuracy for all purposes. In making the comparison, it will be proper to take the medium between the superior and inferior tides of each day, both with respect to time and height, because the difference in these respects between those two tides never entirely disappears.

464. The series of changes which depend on the change of the Moon's declination are of more intricate comparison, because they are so much implicated with the changes depending on her distance. But when freed as much as possible from this complication, and then estimated by the medium between the superior and inferior tide of the same day, they agree extremely well with the theoretical series.

This, by the way, enables us to account for an observation which would otherwise appear inconsistent with the theory, which affirms that the superior tide is greatest when the Moon is in the zenith (392.) The observation is, that on the coasts of France and Spain the tides increase as the Moon is nearer to the equator. But it was shewn in the same article, that in latitudes below 45°, the medium tide increases as the Moon's declination diminishes. Bernoulli justly observes, that the tides with which we are most familiarly acquainted, and from which we form all our rules, must be considered as derived from the more perfect and regular tide formed in the widest part of the Atlantic ocean. Extensive, however, as this may be, it is too narrow for a complete quadrant of the spheroid. Therefore it will grow more and more perfect as its pole advances to the middle of the ocean; and the changes which happen on the bounding coasts, from which the waters are drawn on all sides to make it up, must be vastly more irregular, and will have but a partial resemblance to it. They will, however, resemble it in its chief features. This tide being formed in a considerable southern latitude, it becomes the more certain that the medium tide will diminish as the Moon's declination increases. But although this seeming objection occurs on the French coasts, it is by no means the case on ours, or more to the north. We always observe the superior tide to exceed the inferior, if the Moon have north declination.

The same agreement with theory is observable in the solar tides, or in the effect of the Sun's declination. This indeed is much smaller, but is observed by reason of its regularity. For although it is also complicated with the effects of the Sun's change of distance, this effect having the same period with his declination, one equation may comprehend them both. M. Bernoulli's observation, just mentioned, tends to account for a very general opinion, that the greatest tides are in the equinoxes. I observe, however, that this opinion is far from being well established. Both Sturmy and Colepress speak of it as quite uncertain, and Wallis and Flamstead reject it. It is agreed on all hands, that our winter tides exceed the summer tides. This is thought to confirm that point of the theory which makes the Sun's accumulating force greater as his distance diminishes. I am doubtful of the applicability of this principle, because the approach of the Sun causes the Moon to recede, and her recess is in the triplicate ratio of the Sun's approach. Her accumulating force is therefore diminished in the sesquiplicate ratio of the Sun's approach, and her influence on the phenomena of the tides exceeds the Sun's.

465. The changes arising from the Moon's change of distance are more considerable than those arising from her change of declination. By reason of their implication with those changes, the comparison becomes more difficult. M. Bernoulli did not find it so satisfactory. They are, in general, much less than theory requires. This is probably owing to the mutual effects of undulations which should differ very considerably, but follow each other too closely. In M. de la Place's way of considering the phenomena (to be mentioned afterwards) the diminution in magnitude is very accountable, and, in other respects, the correspondence is greatly improved. When the Moon changes either in perigeo or apogeo, the series is considerably deranged, because the next spring-tide is formed in opposite circumstances. The derangement is still greater, when the Moon is in perigee or apogee in the quadratures. The two adjoining spring-tides should be regular, and the two neap-tides extremely unequal.

466. We shall first consider the changes produced on the times of full sea, and then the changes in the height. M. Bernoulli has computed a table for both the perigean and apogean distance of the Moon, from which it will appear what correction must be made on the regular series. It is computed precisely in the same way as the former, the only difference being in the magnitude of M and S, and we may imitate it by a construction similar to Fig. 49. To make this table of easier use, M. Bernoulli introduces the important observation, that the greatest tide is not, in any part of the world, the tide which happens on the day of new or full Moon, nor even the first or the second tide

after; and that with respect to the Atlantic Ocean, and all its coasts, it is very precisely the third tide. So that should we have high water in any port precisely at noon on the full or change of the Moon, and on the first day of the month, the greatest tide happens at midnight on the second day of the month, or, expressing it in the common way, it is the tide which happens when the Moon is a day and a half old. The summit of the spheroid is therefore 19 or 20 degrees to the eastward of the Sun and Moon. At this distance, the tendency of the accumulating forces of the Sun and Moon to complete the spheroid, and to bring its pole precisely under them, is just balanced by the tendency of the waters to subside. Therefore it is raised no higher, nor can it come nearer to the Sun and Moon, because then the obliquity of the force is diminished, on which the changing power depends. That this is the true cause, appears from this, that it is, in like manner, on the third tide that all the changes are perceived which correspond to the declination of the Moon, or her distance from the Earth. Every thing falls out, therefore, as if the luminaries were 19 or 20 degrees eastward of where they are, having the pole of the spheroid in its theoretical situation with respect to this fictitious situation of the luminaries. But, in such a case, were the Sun and Moon 20° farther eastward, they would pass the meridian 80 minutes, or one hour and 20 minutes later. Therefore $1^h\ 20'$ is added to the hours of high water of the former table, calculated for the mean distance of the Moon from the Earth. Thus, on the day of new Moon, we have not the spring-tide, but the third tide before it, that is, the tide which should happen when the Moon is 20° west of the Sun, or has the elongation 160°. This tide, in our former table, happens at $11^h\ 02'$. Therefore add to this $1^h\ 20'$, and we have $0^h\ 22'$ for the hour of high water on the day of full and change for a harbour which would otherwise have high water when the Sun and Moon are on the meridian.

In this way, by adding $1^h\ 20'$ to the hours of high water in the former table, for a position of the luminaries 20° farther west, it is accommodated to the observed elongation of the Moon, this elongation being always supposed to be that of the Moon when she is on the meridian. Such, then, is the following table of M. Bernoulli. The first column gives the Moon's elongation from the Sun, or from the opposite point of the heavens, the Moon being then on the meridian. The second column gives the hour of high water when the Moon is in perigeo. The third column (which is the same with the former table, with the addition of $1^h\ 20'$) gives the hour of high water when the Moon is at her mean distance. And the fourth column gives the hour when she is apogeo.

TABLE II.

☾ a′ ☉	☾ in Perigeo.	☾ in M. Dist.	☾ in Apogeo.	☾ in Perigeo.	☾ in M. Dist.	☾ in Apogeo.
0°	-.18	-.22	-.27½	After. 18	After. 22	After. 27
10	-.49½	-.51½	-.54	9½	11½	14
20	1.20	1.20	1.20	—	—	—
30	1.50½	1.48½	1.46	9½ Before the M.'s Southing.	11½ Before the M.'s Southing.	14 Before the M.'s Southing.
40	2.22	2.18	2.12½	18	22	27½
50	2.54	2.48½	2.40½	26	31½	39½
60	3.27	3.20	3.10	33	40	50
70	4. 2½	3.55	3.44	37	45	56
80	4.41½	4.33½	4.22	38½	46½	58
90	5.26½	5.19½	5. 9½	33½	40½	50½
100	6.19	6.15	6. 9	22	25	31
110	7.20	7.20	7.20	—	—	—
120	8.21	8.25	8.31	After the M.'s South. 21	After the M.'s South. 25	After the M.'s South. 31
130	9.13½	9.20½	9.30½	33	40	50
140	9.58½	10. 6½	10.18	38	46	58
150	10.37½	10.45	10.56	37	45	56
160	11.13	11.20	11.30	33	40	50
170	11.46	11.51½	11.59½	26	31	39
180	—.18	—.22	—.27½	18	22	27

467. This table, though of considerable service, being far preferable to the usual tide tables, may sometimes deviate a few minutes from the truth, because it is calculated on the supposition of the luminaries being in the equator. But when they have considerable declination, the horary arch of the equator may differ two or three degrees from the elongation. But all this error will be avoided by reckoning the high water from the time of the Moon's southing, which is always given in our almanacks. This interval being always very small (never 12°), the error will be insensible. For this reason, the three other columns are added, expressing the priming of the tides on the Moon's southing.

To accommodate this table to all the changes of the Moon's declination would require more calculation than all the rest. We shall come near enough to the truth, if we lessen the minutes in three hour-columns $\frac{1}{10}$ when the Moon is in the equator, and increase them as much when she is in the tropic, and if we use them as they stand when she is in a middle situation.

468. All that remains now, is to adjust this general table to the peculiar situation of the port. Therefore, collect a great number of observations of the hour of high water at full or change of the Moon. In making this collection, note particularly the hour on those days where the Moon is new or full precisely at noon; for this is the circumstance necessary for the truth of the elongations in the first column of the table. A small equation is necessary for correcting the observed hour of high water, when the syzigy is not at noon, because, in this situation of the luminaries, the tide lags 35′ behind the Sun in a day, as has been already shewn. Suppose the lagging to be 36′, this will make the equation 1½ minute for every hour that the full or change has happened before or after the noon of that day. This correction must be added to the observed hour of high water, if the syzigy was before noon, and sub-

tracted, if it happened after noon. Or, if we choose to refer the time of high water to the Moon's southing, which, in general, is the best method, we must add a minute to the time between high sea and the Moon's southing for every hour and half that the syzigy is before noon, and subtract it if the syzigy has happened after noon. For the tides prime 16' in 24 hours.

469. Having thus obtained the medium hour of high water at full and change of the Moon, note the difference of it from $0^h\ 22'$, and then make a table peculiar to that port, by adding that difference to all the numbers of the columns. The numbers of this table will give the hour of high water corresponding to the Moon's elongation for any other time. It will, however, always be more exact to refer the time to the Moon's southing, for the reasons already given.

By means of a table so constructed, the time of high water for the port, in any day of the lunation, may be depended on to less than a quarter of an hour, except the course of the tides be disturbed by winds or freshes, which admit of no calculation. It might be brought nearer by a much more intricate calculation; but this is altogether unnecessary, on account of the irregularities arising from those causes.

It is not so easy to state, in a series, the variations which happen in the *height* of the tides by the Moon's change of distance, although they are greater than the variations in the *times* of high water. This is partly owing to the great differences which obtain in different ports between the greatest and smallest tides, and partly from the difficulty of expressing the variations in such a manner as to be easily understood by those not familiar with mathematical computations. M. Bernoulli, whom we have followed in all the practical inferences from the physical theory, imagines, that, notwithstanding the great disproportion between the spring and neap tides in different places, and the differences in the absolute magnitudes of both, the middle between the highest and lowest daily variations will proceed in very nearly the same way as in theory. Instead, therefore, of taking the values of M and S, as already established, he takes the height of spring and neap tides in any port as indicative of M + S and M — S for that port. Calling the spring-tide A, and the neap-tide B, this principle will give us $M = \frac{A + B}{2}$, and $S = \frac{A - B}{2}$. From these values of M and S he computes their apogean and perigean values, and then constructs columns of the height of the tides, apogean and perigean, in the same manner as the column already computed for the mean distance of the Moon, that is, computing the parts *mf* and *af* (Fig. 49.) of the whole tide *m a* separately. The same may be done with incomparably less trouble by our construction (Fig. 49.) and the values $M = \frac{A + B}{2}$, and $S = \frac{A - B}{2}$.

Although this is undoubtedly an approximation, and perhaps all the accuracy that is attainable, it is not founded on exact physical principles. The local proportion of A to B depends on circumstances peculiar to the place; and we have no assurance that the changes of the lunar force will operate in the same manner and proportion on these two quantities, however different. We are certain that it will not; otherwise the proportion of spring and neap tides would be the same in all harbours, however much the springs may differ in different harbours. I compared Bernoulli's apogean and perigean tides, in about twenty instances, selected from the observations at Brest and St Malo, where the absolute quantities differ very widely. I was surprised, but not convinced, by the agreement. I am however persuaded, that the table is of great use, and have therefore inserted it, as a model by which a table may easily be computed for any harbour, employing the spring-tide and neap-tide heights observed in that harbour as the A and B for that place. The table is, like the last, accommodated to the easterly deviation of the pole of the spheroid from its theoretical place.

It appears, from this table, and also from the last, that the neap-tides are much more affected by the inequalities of the forces than the spring-tides are. The neap-tides vary from 70 to 128, and the springs from 90 to 114. The first is almost doubled, the last is augmented but ¼.

TABLE III.

Elongation, ☾ & ☉.	HEIGHT OF THE TIDE.		
	☾ in Perigeo.	☾ in M. Dist.	☾ in Apogeo.
0	0,99A+0,15B	0,88A+0,12B	0,79A+0,08B
10	1,10A+0,04B	0,97A+0,03B	0,87A+0,02B
20	1,14A+0,00B	1,00A+0,00B	0,90A+0,00B
30	1,10A+0,04B	0,97A+0,03B	0,87A+0,02B
40	0,99A+0,15B	0,88A+0,12B	0,79A+0,08B
50	0,85A+0,32B	0,75A+0,25B	0,68A+0,18B
60	0,67A+0,53B	0,59A+0,41B	0,53A+0,29B
70	0,46A+0,75B	0,41A+0,59B	0,37A+0,41B
80	0,28A+0,96B	0,25A+0,75B	0,23A+0,53B
90	0,13A+1,13B	0,12A+0,88B	0,11A+0,62B
100	0,03A+1,24B	0,03A+0,97B	0,03A+0,68B
110	0,00A+1,28B	0,00A+1,00B	0,00A+0,70B
120	0,03A+1,24B	0,03A+0,97B	0,03A+0,68B
130	0,13A+1,13B	0,12A+0,88B	0,11A+0,62B
140	0,28A+0,96B	0,25A+0,75B	0,23A+0,53B
150	0,46A+0,75B	0,41A+0,59B	0,37A+0,41B
160	0,67A+0,53B	0,59A+0,41B	0,53A+0,29B
170	0,85A+0,32B	0,75A+0,25B	0,68A+0,18B
180	0,99A+0,15B	0,88A+0,12B	0,79A+0,08B

470. The attentive reader cannot but observe, that all the tables of this monthly construction must be very im-

perfect, although their numbers are perfectly accurate, because, in the course of a month, the declination and distance of the Moon vary, independently of each other, through all their possible magnitudes. The last table is the only one that is immediately applicable, by interpolation. It would require several tables of the same extent, to give us a set of equations, to be applied to the original table of art. 418; and the computation would become as troublesome for this approximation as the calculation of the exact value, taking in every circumstance that can affect the question. For that calculation requires only the computation of two right-angled spherical triangles, preparatory to the calculation of the place of high water. But, with all these imperfections, M. Bernoulli's second table is much more exact than any tide-table yet published.

Such, on the whole, is the information furnished by the doctrine of universal gravitation concerning this curious and important phenomenon. It is undoubtedly the most irrefragable argument that we have for the truth and universality of this doctrine, and, at the same time, for the simplicity of the whole constitution of the solar system, so far as it can be considered mechanically. No new principle is required for an operation of nature so unlike all the other phenomena in the system.

471. The method which I have followed in the investigation is nearly the same with that of its illustrious discoverer. We have contented ourselves with shewing various serieses of phenomena, which tally so well with the legitimate consequences of the theory, that the real source of them can no longer be doubted. And, notwithstanding the various deviations from those consequences, arising from other circumstances, we have obtained practical rules, which make the mariner pretty well acquainted with the general course of the tides, sufficiently to put him on his

guard against the dangers he runs by grossly mistaking them, and even enabling him to take advantage of the course of the tide for prosecuting his voyage. Still, however, a great store of local information is necessary. For there are some parts of the ocean, where the tides follow an order extremely unlike what we have described. The bar of Tonquin in China is one of the most remarkable; and its chief peculiarity consists in its having but one tide in each lunar day. It has been traced to the co-operation of two great tides, coming from opposite quarters, with almost six hours of difference in the time of high water. The result of which is, that the compound tide is the excess of the one above the other, forming a high water when the sum of both their elevations is a maximum. Dr Halley has given a very distinct explanation of this tide in No 162 of the Philosophical Transactions.

472. A very different method of investigating this and a similar phenomenon has been employed by the eminent mathematicians, D'Alembert and La Place, in which M. La Place, who makes this a chief article of his Mechanique Celeste, deduces the whole directly from the interior mechanism of hydrostatical undulations. His main inferences perfectly agree with those already delivered. The method of Newton and Bernoulli has been preferred here, because by this means the connexion with the operation of universal gravitation is much better kept in sight. At the same time, La Place's method allows us, in some cases, to state the individual fact more nearly as it occurs, without considering it as the modification of another fact that is more general. But it may be doubted, whether La Place has explained all the variety of phenomena. His whole application is limited by the data which furnish the arbitrary quantities in his equations. These being wholly taken from the observations in the ports of France and Spain, it may be questioned whether the sameness, arising from the latitude being so near 45°, may not have made the inge-

nious author simplify too much his theory. He considers every class of phenomena as operations completely accomplished, and the ocean at the end of the action of any one of the forces as in a state of indifference, ready for the free operation of the next. For example, the equality of the superior and inferior tides of one day is deduced by La Place immediately from the circumstance of the ocean being of nearly an uniform depth, saying that the small inferior tide is not affected by the greatness of the preceding superior tide, because the obstructions are such, that all motions cease very soon, almost immediately after the force has ceased to act. We doubt the truth of the near uniformity of the sea's depth. The unequal tides are confessedly most remarkable on the coasts, where the depth is the most unequal. The other principle, that the effects of primitive motions are all obliterated, and therefore every tide is the completed operation of the present force, is still more questionable. It is well known, that the roll of a great storm in the Bay of Biscay is very sensible indeed for three days. Of this we have had repeated experience. The *superficial* agitation of a storm (for it is no more) is nothing in comparison with the huge uniform momentum of a tide; and the greatest storm, even while it blows, cannot raise the tide three feet; nor does it even then change what we have called the tide, the difference between high and low water; it raises or keeps down both nearly alike. Besides, how will M. La Place account for the undeniable duration of every tide-wave on the coasts of Europe and America for a day and a half? There can be no question about this, because the course of the tides during a month is precisely conformable to it. The tide which bears the mark of the perigean tide is not the tide which happens when the Moon is in perigeo, but the third following that tide, just as in the springs and neaps. In like manner, it is observed at Brest, without one exception for six years, that the morning or superior tide at new Moon is smaller than the inferior tide in summer. In winter it is the contrary, not, however, with such constant accuracy. Now, it should be just the contrary, if the tides observed were the tides corresponding with the then state of the forces. But they are not. They are tides corresponding with the state of the forces thirty-six hours before. (See Mem. Acad. Par. 1720, p. 206, duodecimo.) It is the same at full Moon, that is, the morning tide in summer is less than the evening tide. The morning tide corresponding to the then state of the forces is what we have called an inferior tide, the Moon being then under the horizon, with south declination. The tide, therefore, should be greater than the subsequent or evening, or superior tide. But, like the last example, it is the tide corresponding to the forces in action thirty-six hours before. Can we now deny that the present state of the waters is affected by the action of forces which have ceased thirty-six hours ago? and if this be granted, it is impossible that two tides immediately succeeding can be very unequal. The contrary can be shewn in an experiment perfectly resembling the great tides of the ocean. An apparatus, made for exhibiting the appearance of a reciprocating spring, was so constructed, that one of its runnings was very sudden and copious, and the next was moderate and slow. It emptied into a small basin, which communicated with a long and narrow horizontal channel, shut at the far end, the basin emptying itself by a small spout on the opposite side. Thus, two very unequal floods and ebbs presented themselves at the mouth of this channel, and sent a wave along it, which, at the first, was very unequal. But when it was mixed with the returning wave from the far end, they were soon brought to an apparent equality. The experiment appearing curious, it was prosecuted, by various changes of the apparatus; and several effects tended very much to explain some of the more singular appearances of the tides. There is an example of the continuance of former impressions in the tides among the western islands of Scotland, that considerably resembles the tide on the bar of Tonquin. The general course of the flood round the little island of Berneray is N.E. and that of the ebb is S.W. But at a certain time in the spring, both flood and ebb run N.E. during twelve hours, and the next flood and ebb run S.W. The contrary happens in autumn. Yet in the offing, the flood and ebb hold their regular courses. This greatly resembles the tide at Tonquin, and also the Grecian Euripus.

473. The reader will recollect that we stated, as our opinion, that, in consequence of the inertia of the waters, the pole of the ocean is always to the eastward of its theoretical place. For which reason, the figure actually attained by the ocean is not a figure of equilibration. Did the Earth stand still, it would soon be brought to its proper position, and completed to its due form. Therefore, there is always a motion *towards* this completion; *and this motion is obstructed.* Hence we apprehend, that there must be a perpetual current of the waters, especially in the tropical regions, from east to west. We cannot see how this can be avoided; and we think that it is established as a matter of nautical observation. In regard to the Atlantic, this seems to be a general opinion of the navigators. There are two very excellent journals of voyages from Stockholm to China, by Captain Eckhart, in which there is a very frequent comparison of the ship's reckoning with lunar observations and the arrivals on known coasts, from which we cannot help inferring the same general current in the Indian and Ethiopic seas. It seems, therefore, to obtain over the whole. The part of this current which diffuses itself into the Atlantic is but small, it having a freer passage straight forward. But the part thus diffused produces the gulf stream, in its way along the American coasts, and escapes round the north capes of Europe and America. In all probability, a south-

erly current may be observed in the straits which separate America from the Asiatic continent. The whole amount of this motion cannot be considerable, but there must be some, if there be two circumpolar communications between the great eastern and western divisions of the ocean. Without this, it must be reduced to a reciprocating motion too intricate for investigation.

474. There is another circumstance which seems to strengthen our confidence in the reality of this westerly current of the ocean. The gravity of the waters being more diminished in conjunction and opposition than it is augmented in quadrature with the acting luminary, each particle tends to recede from the centre, and to describe a larger circle, employing a longer time. Here is a tendency or *nisus* to a relative motion westerly. Water, being almost perfectly fluid, will obey this tendency, and in time acquire such a motion, were it not obstructed by solid obstacles. But some effect must remain, too intricate to admit any calculation, and perhaps not ultimately sensible.

475. If the height of the atmosphere be equal to the radius of the Earth, we shall have a tide in the air double of that in the ocean. When all the affecting circumstances are considered, it appears that an ebb and flood of the atmosphere may differ in elevation about 120 feet. This might be sensible by affecting the barometer. True, the gravity of the mercury is also diminished, but not so much as that of the more distant air. But the height of the atmosphere is too small to give rise to any such tides. They cannot sensibly exceed those of the ocean, and this cannot change the height of the mercury in the barometer $\frac{2}{100}$ of an inch. Professor Toaldo at Padua kept a register of the barometer for more than thirty years. He has added into one sum all the mercurial heights observed at new Moon. Another sum was made of all the heights observed in the quadratures; another of the perigean; and another of the apogean heights, &c. &c. He thinks that differences were

observed in those sums sufficient for proving the accumulation and compression of the air by its unequal gravitation to the Moon. Thus the apogean heights exceeded the perigean by 14 inches. The heights in syzigy exceeded those in quadrature by 11 inches. (See Mem. Berlin, 1777, and a book expressly on the subject.)

But there is another effect of this disturbing force which may be much more sensible, namely, the general westerly current of the air. M. D'Alembert has investigated this with great care, and singular address, and has proved that there must be a westerly current in the tropical regions, at the rate of eight feet nearly in a second. This is a very adequate cause of the trade winds which are observed between the tropics. It is indeed increased by the rarefaction of the air occasioned by the heat of the Sun, which expands the air heated by the ground, and it is both raised and diffused latterly. When the Sun has passed the meridian a proper number of degrees, the air must now cool, and in cooling contract behind the Sun. Air from the east comes in greater abundance than from any other quarter to supply the vacancy.

476. The disk of Jupiter, when viewed through a good telescope, is distinguishable into zones, like a bit of striped satin. These zones, or belts, are of changeable breadth and position, but all parallel to his equator. Therefore they are not attached to his surface, but float on it, as clouds float on our atmosphere. This Earth will have somewhat of this appearance, if viewed from the Moon; for each climate has a state of the sky peculiar in some degree to itself in this respect, and there must be a sort of sameness in one climate all round the globe. A series of observations on a particular spot of Jupiter's surface demonstrate his rotation in 9^h 56′. Spots have been observed in the belts, which have lasted so long as to make several revolutions before they were effaced. They appear to require a minute or two more for their rotation, and therefore have a

westerly motion relative to the firm surface of the planet. This, however, cannot be depended on from the time of their rotation. But a few observations have been had of spots in the vicinity of the fixed spot of his surface, and here the relative motion westward was distinctly observed. M. Schroeter at Manheim has observed the atmosphere of Jupiter with great care, and finds it exceedingly variable; and spots are observed to change their situations with amazing rapidity, with great irregularity, but most commonly eastward. The motions and changes are so rapid, and so extensive, that we can scarcely consider them as the transference of matter from one place to another. They more resemble the changes which happen in our atmosphere, which are sometimes progressive, over a great tract of the country. The storm in 1772 was felt from Siberia to America in succession. The gale blew from the west, but the chemical operation which produced it was in the opposite direction, being first observed in Siberia. Three days afterward, it was felt at St Petersburg; two days after this, at Berlin; two days more, it was in Britain; and seven days after, it was felt in North America. Here then, while a spectator on the earth saw the clouds moving to the eastward, a spectator in the Moon would see the change of appearance proceed from east to west. The motions in the atmosphere of Jupiter must be very complicated, because they are the joint operation of four satellites. The inequality of gravitation to the first satellite must be very great. And as each satellite produces a peculiar tide, the combination of all their actions must be very intricate. We can draw no conclusions from the variable spots, because their change of place is no proof of the actual transference of matter.

Such a relative motion in our atmosphere and in the ocean may affect the rotation, retarding it, by its action on the eastern surface of every obstacle. Yet no change is observed. The year, and the periods of the planets, in the time of Ptolemy, are the same with the present, that is, contain the same number of rotations of the Earth. Perhaps a compensation is maintained by this means for the acceleration that should arise from the transference of soil from the high land to the bottom of the sea, where it is moving round the axis with diminished velocity.

477. With this we conclude our account of physical astronomy—a department of natural philosophy which should ever be cherished with peculiar affection by all who think well of human nature. There is none in which the access to well-founded knowledge seems so effectually barred against us; and yet there is none in which we have made such unquestionable progress—none in which we have acquired knowledge so uncontrovertibly supported, or so complete. How much therefore are we indebted to the man who laid the magnificent scene open to our view, and who gave us the optics by which we can examine its most extensive and its most minute parts! For Newton not only taught us all that we know of the celestial mechanism, but also gave us the mathematics, without which it would have remained unseen.

> " Tu Pater et rerum Inventor. Tu patria nobis
> " Suppeditas præcepta, tuisque ex inclyte chartis
> " Floriferis ut apes in saltibus omnia libant,
> " Omnia nos itidem depascimur aurea dicta
> " Aurea, perpetuâ semper dignissima vitâ."
>
> Lucretius.

For surely the lessons are precious by which we are taught a system of doctrine which cannot be shaken, or share that fluctuation which has attached to all other speculations of curious man. But this cannot fail us, because it is nothing but a well-ordered narration of facts, presenting the events of nature to us in a way that at once points out their subordination, and most of their relations. While the magnificence of the objects commands respect, and perhaps raises our opinion of the excellence of human reason as high as is justifiable, we should ever keep in mind that Newton's success was owing to the modesty of his procedure. He peremptorily resisted all disposition to speculate beyond the province of human intellect, conscious that all attainable science consisted in carefully ascertaining nature's own laws, and that every attempt to explain an ultimate law of nature, by assigning its cause, is absurd in itself, against the acknowledged laws of judgment, and will most certainly lead to error. It is only by following his example that we can hope for his success.

It is surely another great recommendation of this branch of natural philosophy, that it is so simple. One single agent, a force decreasing as the square of the distance increases, is, of itself, adequate to the production of all the movements of the solar system. If the direction of the projection do not pass through the centre of gravity, the body will not only describe an ellipse round the central body, but will also turn round its axis. By this rotation, the body will alter its form; but the same power enables it to assume a new form, which is perfectly symmetrical, and is permanent. This new form, however, in consequence of the universality of gravitation, induces a new motion in the body, by which the position of the axis is slowly changed, and the whole host of heaven appears to the inhabitants of this Earth to change its motions. Lastly, if the revolving planet have a covering of fluid matter, this fluid is thrown into certain regular undulations, which are produced and modified by the same power.

Thus we see that, by following the simple fact of gravitation of every particle of matter to every other particle, through all its complications, we find an explanation of almost every phenomenon of the solar system that has engaged the attention of the philosopher, and that nothing more is needed for the explanation. Till we were put on this track of investigation, these different movements were solitary facts; and, being so extremely unlike, the wit of man would certainly have attempted to explain them by causes equally dissimilar. The happy detection of this simple and easily-observed principle, by a genius qualified for following it into its various consequences, has freed us from numberless errors, into which we must have continually run while pertinaciously proceeding in an improper path. But this detection has not merely saved us from errors, but, which is most remarkable, it has brought into view many circumstances in the phenomena themselves, many peculiarities of motion, which would never have been observed by us, had we not gotten this monitor, pointing out to us where to look for peculiarities. We should never have been able to predict, with such wonderful precision, the complicated motions of some of the planets, had we not had this key to all the equations by which every deviation from regular elliptical motion is expressed.

On all these accounts, physical astronomy, or the mechanism of the celestial motions, is a beautiful department of science. I do not know any body of doctrine so comprehensive, and yet so exceedingly simple; and this consideration made me the more readily accede to those reasons of scientific propriety which point it out as the first article of a course of mechanical philosophy. Its simplicity makes it easy, and the exquisite agreement with observation makes it a fine example of the truth and competency of our dynamical doctrines.

478. But it has other recommendations, of a far greater value. Nothing surely so much engages a heart possessed of a proper sensibility, as the contemplation of order and harmony. No philosophy is requisite for being susceptible of this impression. We see it influence the conduct of the most uncultivated. What else does man aim at in all the bustle of cultivated society? Nay, even the savage makes some rude aim at order and ornament.

But what we contemplate in the solar system is something more than mere order and symmetry, such as may be observed in a fine specimen of crystallization. The order of the solar system is made up of many palpable *subserviences*, where we see one thing plainly done for the sake of another thing. And, to render this still more interesting, a manifest *utility* appears in every circumstance of the constitution of the system, as far as we understand its applicability to what we conceive to be useful purposes. We can mean nothing by utility but the subserviency to the enjoyments of sentient beings. Our opportunities for observations of this kind are no doubt very limited, confined to our own sublunary habitation. But this circumscribed scene of observation is even crowded with examples of utility. Surely it is unnecessary to recall our attention to the numberless adaptations of the systematic connexion with the Sun and Moon to the continuance and the diffusion of the means of animal life and enjoyment. As our knowledge of the celestial phenomena is enlarged, the probability becomes stronger that other planets are also stored with inhabitants who share with us the Creator's bounty. Their rotation, and the evident changes that we see going on in their atmospheres, so much resemble what we experience here, that I imagine that no man, who clearly conceives them, can shut out the thought that these planets are inhabited by sentient beings. And there is nothing to forbid us from supposing that there is the same inexhaustible store of subordinate contrivance for their accommodation that we see here for living creatures in every situation, with appropriate forms, desires, and abilities. I fear not to appeal to the heart of every man who has learned so much of the celestial phenomena, even the man who scouts this opinion, whether he does not feel the disposition to entertain it. And I insist on it, that some good reason is required for rejecting it.

479. When beholding all this, it is impossible to prevent the surmise, at least, of purpose, design, and contrivance, from arising in the mind. We may try to shut it out—We may be convinced, that to allege any purpose as

an argument for the reality of any disputed fact, is against the rules of good reasoning, and that final causes are improper topics of argument. But we cannot hinder the anatomist, who observes the exquisite adaptation of every circumstance in the eye to the forming and rendering vivid and distinct a picture of external objects, from believing that the eye was made for seeing—or the hand for handling. Neither can we prevent our heart from suggesting the thought of transcendent wisdom, when we contemplate the exquisite fitness and adjustment which the mechanism of the solar system exhibits in all its parts.

480. Newton was certainly thus affected, when he took a considerate view of all his own discoveries, and perceived the almost eternal order and harmony which results from the simple and unmixed operation of universal gravitation. This single fact produces all this fair order and utility. Newton was a mathematician, and saw that the law of gravitation observed in the system is the only one that can secure the continuance of order. He was a philosopher, and saw that it was a contingent law of gravitation, and might have been otherwise. It therefore appeared to Newton, as it would to any unprejudiced mind, a law of gravitation selected as the most proper, out of many that were equally possible; it appeared to be a choice, the act of a mind, which comprehended the extent of its influence, and intended the advantages of its operation, being prompted by the desire of giving happiness to the works of almighty power.

Impressed with such thoughts, Newton breaks out into the following exclamation, ‘ *Elegantissima hæcce compages* ‘ *Solis Planetarum et Cometarum, non nisi consilio et do-* ‘ *minio Entis cujusdam potentis et intelligentis oriri po-* ‘ *tuit. Hæc omnia regit, non ut anima mundi, sed ut uni-* ‘ *versorum Dominus mundorum. Et propter dominium* ‘ *Dominus Deus,* Παντοκρατωρ, *dici solet. Deitas est domi-*

'*natio Dei, non in corpus proprium, uti sentiunt quibus* '*Deus est anima mundi, sed in servos,*' &c.

These were the effusions of an affectionate heart, sympathising with the enjoyment of those who shared with him the advantages of their situation. Yet Newton did not know the full extent of the harmony that he had discovered. He thought that, in the course of ages, things would go into disorder, and need the restoring hand of God. But, as has been already observed, De la Grange has demonstrated that no such disorder will happen. The greatest deviations from the most regular motions will be almost insensible, and they are all periodical, waning to nothing, and again rising to their small maximum.

481. These are surely pleasing thoughts to a cultivated mind. It is not surprising therefore that men of affectionate hearts should too fondly indulge them, and that they should sometimes be mistaken in their notions of the purposes answered by some of the infinitely varied and complicated phenomena of the universe. And it would be nothing but what we have met with in other paths of speculation, should we see them consider a subserviency to this fancied purpose as an argument that an operation of nature is effected in one way, and not in another. In this way, the employment of final causes has sometimes obstructed the progress of knowledge, and has been productive of error. But the impropriety of this kind of argumentation proceeds chiefly from the great chance of our being mistaken with respect to the aim of nature on the occasion. Could this be properly established as a fact, and could the subserviency of a precise mode of accomplishing a particular operation be as clearly made out, I apprehend that, however unwilling the logician may be to admit this as a good reason, he cannot help feeling its great force. That this is true, is plain from the rules of evidence that are admitted in all courts; where a purpose being proved, the subserviency of a certain deed to that purpose is allowed to be evidence that

this was the intention in the commission of that deed. It is, however, very rarely indeed that such argument can be used, or that it is wanted, and it never supersedes the investigation of the efficient cause.

482. But speculative men have of late years shewn a wonderful hostility to final causes. Lord Bacon had said, more wittily than justly, that all use of final causes should be banished from philosophy, because, 'like Vestals, they produce nothing.' This is not historically true; for much has been discovered by researches conducted *entirely* by notions of final causes. What other evidence have we for all that we know concerning the nature of man? Is not this a part of the book of Nature, and some of its most beautiful pages? We know them only by the appearances of design, that is, by the adaptations of things in evident subserviency to certain results. Are there no such adaptations to be seen, except in the works of man? Nature is crowded with them on every hand, and some of her most important operations have been ascertained by attending to them. Dr Harvey discovered the circulation of the blood in this very way. He saw that the valves in the arteries and veins were constructed precisely like those of a double forcing pump, and that the muscles of the heart were also fitted for an alternate systole and diastole, so corresponding to the structure of those valves, that the whole was fit for performing such an office. With boldness therefore he asserted that the beatings of the heart were the strokes of this pump; and, laying the heart of a living animal open to the view, he had the pleasure of seeing the alternate expansion and contractions of its auricles and ventricles, exactly as he had expected. Here was a discovery, as curious, as great, as important, as universal gravitation. In precisely the same way have all the discoveries in anatomy and physiology been made. A new object is seen. The discoverer immediately examines its structure—why? To see what it can perform; and if he sees a number of coadaptations to

a particular purpose, he does not hesitate to say, 'this is its purpose.' He has often been mistaken; but the mistakes have been gradually corrected—how? By discovering what is the real structure, and what the thing is really fit for performing. The anatomist never imagines that what he has discovered is of no use.*

486. So far therefore from banishing the consideration of final causes from our discussions, it would look more like philosophy, more like the love of true wisdom, and it would taste less of an idle curiosity, were we to multiply our researches in those departments of nature where final causes are the chief objects of our attention—the structure and economy of organised bodies in the animal and vegetable kingdoms. I cannot help remarking, with regret, that of late years, the taste of naturalists has greatly changed, and, in my humble opinion, for the worse. The study of inert matter has supplanted that of animal life. Chemistry and mineralogy are almost the sole objects of attention. Nay, the *ruins* of nature, the shattered relics of a former world, seems a more engaging object than the numberless beauties that now adorn the present surface of our globe. I acknowledge that, even in those inanimate works, God has not left himself without a witness. Yet surely we do not, in the bowels of the Earth, nor even in the curious operations of chemical affinity, see so palpably, or so pleasantly, the incomprehensible wisdom and the providential beneficence of the Father of all, as in the animated objects of nature.

It is not easy to account for it, and perhaps the explanation would not be very agreeable, why many naturalists so fastidiously avoid such views of nature as tend to lead the mind to the thoughts of its Author. We see them even anxious to weaken every argument for the appearance of design in the construction and operations of nature. One should think, that, on the contrary, such appearance would be most welcome, and that nothing would be more dreary and comfortless than the belief that chance or fate rules all the events of nature.

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* I would earnestly recommend to my young readers some excellent remarks on the argument of final causes (without which Cicero thought that there is no philosophy) in the preface by the editor of Derham's Physico-Theology, published at London in 1798. He there considers the proper province of this argument, its use, and incautious abuse, with the greatest perspicuity and judgment.

484. I have been led into these reflections by reading a passage in M. de la Place's beautiful Synopsis of the Newtonian Philosophy, published by him in 1796, under the title of *Systême du Monde*. In the whole of this work, the author misses no opportunity of lessening the impression that might be made by the peculiar suitableness of any circumstance in the constitution of the solar system to render it a scene of habitation and enjoyment to sentient beings, or which might lead the mind to the notion of the system's being contrived for any purpose whatever. He sometimes, on the contrary, endeavours to shew how the alleged purpose may be much better accomplished in some other way. He labours to leave a general impression on the mind, that the whole frame is the necessary result of the primitive and essential properties of matter, and that it could not be any thing but what it is. He indeed concludes, like the illustrious Newton, with a survey of all that has been done and discovered, followed by some reflections suggested (as he says) by this survey.

"Astronomy," says M. de la Place, "in its present "state, is unquestionably the most brilliant specimen of the "powers of the human understanding." He does not, however, tell us this is so manifest. He does not say how that this object, which has engaged, and so properly occupied this fine understanding, has any thing to justify the choice, either on account of its beautiful symmetry, or exquisite contrivance, or multifarious utility; or, in short, that it is an object that is worth looking at. But he gives us to understand, that astronomy has now taught us how much we were mistaken, in thinking ourselves an important part of the universe, for whose accommodation much has been done, as if we were objects of peculiar care. But we have been punished, says he, for these mistaken notions of self-importance, by the foolish anxieties to which they have given rise, and by the subjugation to which we have submitted, while under the influence of these superstitious terrors. Mistaking our relations to the rest of the universe, social order has been supposed to have other foundations than justice and truth, and an abominable maxim has been admitted, that it was sometimes useful to deceive and to subdue mankind, in order to secure the happiness of society. But nature resumes her rights, and cruel experience has shewn that she will not allow those sacred laws to be broken with impunity.

485. I think it will require some investigation before we can find out what connexion there is between the discoveries of Sir Isaac Newton, and this mysterious detection that M. de la Place has at last deduced from the survey. It is communicated in the dark words of an oracle, and we are left to interpret for ourselves. I can affix no meaning but this, that ignorance and self-conceit have made us imagine that this Earth is the centre, and the principal object of the universe, and that all that we see derives its value from its subserviency to this Earth, and to man its chief inhabitant. We fondly imagined, that we are the objects of peculiar care,—that it is for us that the magnificent spectacle is displayed,—and that our fortunes are to be read in the starry heavens. But it is now demonstrated that this Earth, when compared, even with some single objects of our system, is but like a peppercorn. The whole system is but as a point in the universe. How insignificant then are we! But we have been justly punished for our self-conceit, by imagining that the stars influence our fortunes, and have

made ourselves the willing dupes of astrologers and soothsayers.

Thus far I think that M. de la Place's words have some meaning, but, surely, very little importance; nor did it call for any congratulatory address to his contemporaries on their emancipation from such fears. It is more than a century since all thoughts of the central situation and great bulk of the Earth, and of the influence of the stars on human affairs, have been exploded and forgotten.

But the remaining part of the remarks, about social order, and truth, and justice, and about deceiving and enslaving mankind, in order to secure their happiness, is more mysterious. " More is meant than meets the ear." M. de la Place carefully abstains, through the whole of this performance, from all reference to a Contriver, Creator, or Governor of the Universe, particularly in the present reflections, *which are so pointedly contrasted* with the concluding reflections of the great Newton. The opposition is so remarkable, that it startles every reader who has perused the Principia. I cannot but suspect that M. de la Place would here insinuate that the doctrine of a Deity, the Maker and Governor of this World, and of his peculiar attention to the conduct of men, is not consistent with truth; and that the sanctions of religion, which have long been venerated as the great security of society, are as little consistent with justice. The duties which we are said to owe to this Deity, and the terrors of punishment in a future state of existence for the neglect of them, have enabled wicked men to enslave the world, subjecting mankind to an oppressive hierarchy, or to some temporal tyrant. The priesthood has, in all ages and nations, been the great support of the despot's throne. But now man has resumed his natural rights. The throne and the altar are overturned, and truth and justice are the order of the day.

486. This is by no means a groundless interpretation of De la Place's words. He has given abundant proofs of

these being his sentiments. It accords completely with his anxious endeavours, on all occasions, to flatten or depress every thing that has the appearance of order, beauty, or subserviency, and to resolve all into the irresistible operation of the essential properties of matter.

487. Of all the marks of purpose and of wise contrivance in the solar system, the most conspicuous is the selection of a gravitation in the inverse duplicate ratio of the distances. Till within these few eventful years, it has been the professed admiration of philosophers of all sects. Even the materialists have not always been on their guard, nor taken care to suppress their wonder at the almost eternal duration and order which it secures to the solar system. But M. de la Place annihilates at once all the wisdom of this selection, by saying, that this law of gravitation is essential to all qualities that are diffused from a centre. It is the law of action inherent in an atom of matter in virtue of its mere existence. Therefore, it is no indication of purpose, or mark of choice, or example of wisdom. It cannot be otherwise. Matter is what it is.

M. de la Place was aware that this assertion, so contrary to a notion long and fondly entertained, would not be admitted without some unwillingness. He, therefore, gives a demonstration of his proposition. He compares the action of gravity, at different distances, with the illumination of a surface placed at different distances from the radiant point. Thus, let light, diffused from the point A (Fig. 54.) shine through the hole B C D E, which we shall suppose an inch square, and let this light be received on a surface *b c d e* parallel to the hole, and twice as far from A. We know that it will illuminate a surface of four square inches. Therefore, since all the light which covers these four inches, came through a hole of one inch, the light in any part of the illuminated surface is four times weaker than in the hole, where it is four times denser. In like manner, the intensity, and efficiency of any quality diffused from A, and operating at twice the distance, must be four times less or weaker; and at thrice the distance it must be nine times weaker, &c. &c.

488. But there is not the least shadow of proof here, nor any similarity, on which an argument may be founded. We have no conception of any degrees or magnitude in the intensity of any such quality as gravitation, attraction, or repulsion, nor any measure of them, except the very effect which we conceive them to produce. At a double distance, gravity will generate one-fourth of the velocity in the same time. But this measure of its strength or weakness has no connexion whatever with density, or figured magnitude, on which connexion the whole argument is founded. What can be meant by a double density of gravity? What is this density? It is purely a geometrical notion; and in our endeavour to conceive it with some distinctness, we find our thoughts employed upon a *certain determined number* of lines spreading every way from the radiant point, and passing through the hole B C D E at equal distances among themselves. It is very true that *the number* of those lines which will be intercepted by a given surface at twice the distance will be only one-fourth of the number intercepted by the same surface at the simple distance. But I do not see how this can apply to the intensity of a mechanical force, unless we can consider this force as an effect, and can show the influence of each line in producing the effect which we call the force, and which we consider as the cause of the phenomenon called gravitation. But if we take this view of it, it is no longer an example of his proposition—a force diffused from a centre. For, in order to have the efficiency inversely as the square of the distance, it is measured by the number of efficient lines intercepted. Here it is plain that the efficiency of one of those lines is held to be equal at every distance from the centre. Such incongruity is mere nonsense.

This conception of a bundle of lines is the sole foundation for any argument in the present case. La Place indeed tries to avoid this by a different way of expressing his example. A certain quantity of light, says he, goes through the hole. This is uniformly spread over four times the surface, and must be four times thinner spread. But this, besides employing a gratuitous notion of light, which may be refused, involves the same notion of *discrete* numerical quantity. If light be not conceived to consist of atoms, there can be no difference of density; and if we consider gravity in this way, we get into the hypothesis of mechanical impulsion, and are no longer considering gravity as a primordial force or quality.

489. But this pretended demonstration is still more deficient in metaphysical accuracy. The proposition to be demonstrated is, that the gravitation towards an atom of matter is in the inverse duplicate ratio of the distance, *in whatever point of space the gravitating atom is placed.* But if we take our proof of the ratio from the conception of these lines, and their density, we at once admit that there are an infinity of situations in which there is no gravitation at all, namely, in the intervals of these lines. The number of situations in which the atom gravitates is a mere nothing in comparison with those in which it does not. We must either suppose that both the quality and the surface influenced by it are continuous, uninterrupted,—or both must be conceived as *discrete* numerical quantities, the quality operating along a *certain number* of lines, and the surface consisting of a *certain number* of points. We must take one of these views. But neither of them gives us any conception of a different energy at different distances. If the surface be *continuous*, and the quality *every where* operative, there can be no difference of effect, unless we at once admit that the energy itself changes with the distance. But this change can have no relation to a change of density, a thing altogether inconceivable in a continuous substance;—where every place is full, there can be no

more. On the other hand, if the quality be exerted only along certain lines, and the surface only contain a certain number of points, we can find no ground for establishing any proportion.

490. The simple and true state of the question is this. Suppose only two indivisible atoms, or two mathematical points of such atoms, in the universe. If these atoms be supposed to attract each other, *wherever they are placed*, do we perceive any thing in our conception of this force that can enable us to say that the attraction is equal or unequal, at different distances? For my own part I know nothing. The gravitation, and its law of action, are mere phenomena, like the thing which I call matter. This is equally unknown to me. I merely observe certain relations, which have hitherto been constant, and I am led by the constitution of my mind to expect the continuation of these relations. My collection of such observations is my knowledge of its nature. This gravitation is one of them, and this is all that I know about it.

491. The observed relations may be such that they involve certain consequences. This, in particular, has consequences that cannot be disputed. If gravitation in the ratio of $\frac{1}{x^2}$ be the primordial relation of all matter, and the source of all others (which is part of La Place's system), it is impossible that a particle composed of such atoms can act with a force which decreases more rapidly by an increase of distance; but there are many phenomena which indicate a much more rapid decrease of force. Simple cohesion of solid bodies is one of these. The expansion of some exploding compositions shew the same thing. We may add, that no composition of such atoms can form repelling particles, nor give rise to many expansive fluids, or indeed to any of the ordinary phenomena of elastic bodies. But these things are not immediately before us, and we

shall have another and a better opportunity of considering many things connected with this great question.

492. De la Place is not the first person who has attempted a demonstration of this proposition. Dr David Gregory, in his valuable work on astronomy, has done the same thing, and nearly in the same way with La Place. Leibnitz, in that strange letter to the editors of the Leipzig Review, in which he answers some of Gregory's objections to his own theory of the celestial motions, mentions an Italian professor who gave the same argument, and affected to consider this ratio of planetary force as known to him before Newton's discovery. Leibnitz thinks the argument a very good one, because, mathematically speaking, it is the same thing whether the rays be illuminative or attractive. If this be not nonsense, I do not know what is.—Several compilers of elements employ the same argument; but nothing can be less to the purpose. Nothing can be more illogical than to speak of demonstrating any primordial quality. Newton was surely more interested in this question than any other person; and we may be certain that if he could have supported his discovery of this law of gravitation by any argument from higher principles, he most certainly would have done it. But there is no trace of any attempt of the kind among his writings—doubtless because he saw the folly of the attempt.

493. I trust that the reader will forgive me for taking up so much of his time with this question. It seems to me of primary importance. Charged as I am with the instruction of youth—the future hopes of our country—it is my bounden duty to guard their minds from every thing that I think hazardous. This is the more incumbent on me, when I see natural philosophy calumniated, and accused of lending her support to doctrines which are the abhorrence of all the wise and good. I cannot better discharge this duty than by wiping off this stain, with which careless ig-

norance, or atheistical perversion, has disfigured the fair features of philosophy. I was grieved when I first saw M. de la Place, after having so beautifully epitomised the philosophy of Sir Isaac Newton, conclude his performance with such a marked and ungraceful parody on the closing reflections of our illustrious master; and, as I warmly recommend this epitome to my pupils, it became the more necessary to take notice of the reprehensible peculiarities which occur in different parts of the work; and particularly of this proposition, from which the materialists seem to entertain such hopes.

It is somewhat amusing to remark how the authority of Sir Isaac Newton has been eagerly catched at by the atheistical sophists to support their abject doctrines. While some hankering remained in France for the Atomistic philosophy, and there was any chance of bewildering the imaginations, and misleading the understandings, of such as wished to acquire a confident faith in the reveries of Democritus and Epicurus, M. Diderot worked into a better shape the slovenly performance of Robinet, the *Systême de la Nature*, and affected to deduce all his vibrations and vibratiuncles from the elastic æther of Sir Isaac Newton, dressing up the scheme with mathematical theorems and corollaries. And thus, Newton, one of the most pious of mankind, was set at the head of the atheistical sect.

But this mode, having had its day, is now passed, and is become obsolete—the tide has completely turned, and the æther is no longer wanted. But the sect would not quit their hold of Sir Isaac Newton. The doctrine of universal fate is now founded on Newton's great discovery of gravitation in the inverse duplicate ratio of the distances. It is still called the discovery of the illustrious Englishman, and is passed from hand to hand with all the authority of his name.

494. But surely to us, the scholars of Newton, the futi-

lity of this attempt is abundantly manifest. As the worthy pupils of our accomplished teacher, we will join with him in considering universal gravitation as a noble proof of the existence and superintendence of a SUPREME MIND, and a conspicuous mark of ITS transcendent wisdom. The discovery of this relation between the particles of that matter of which the solar system consists is acknowledged, even by the materialists, to have set Newton at the head of philosophers. They must therefore grant that it has something in it of peculiar excellence. Indeed whoever is able to follow the steps of Newton over the magnificent scene, must be affected as he was, and must pronounce 'all very good.' It is peculiarly deserving of remark, that we see many contrivances in this system, which are of manifest subserviency to the enjoyments of man, and which do not appear to have any farther importance. Man is unquestionably the lord of this lower world, and all things are placed under his feet. But we see nothing to which man is exclusively subservient—nothing that is superior to man in excellence, so far as we can judge of what is excellent—nothing but that wisdom, that power, and that beneficence, which seem to indicate and to characterise the Author and Conductor of the whole;—and, I may add, that it is not one of our smallest obligations to the Author of Nature, that He has given us those powers of mind which enable us to perceive and to be delighted with the sight of this bright emanation of all his perfections.

> " Sanctius his animal, mentisque capacius altæ,
> " Finxit in effigiem moderantûm cuncta Deorum,
> " Pronaque cum spectent animalia cætera terram,
> " Os homini sublime dedit, cœlumque tueri
> " Jussit, et erectos ad sidera tollere vultus."
>
> OVID.

Allow me to conclude in the words of Dr Halley:

"Talia monstrantem mecum celebrate Camœnis,
"Vos, ô cœlicolúm gaudentes nectare vesci,
"NEWTONUM, clausi reserantem scrinia Veri,
"NEWTONUM, Musis charum, cui pectore puro
"Phœbus adest, totoque incessit Numine mentem,
"Nec fas ese propiùs mortali attingere divos."

HALLEY.

TELESCOPE.

TELESCOPE, an optical instrument for viewing distant objects; so named by compounding the Greek words τηλε *far off*, and σκοπειν *to look at* or *contemplate*. This name is commonly appropriated to the larger sizes of the instrument, while the smaller are called PERSPECTIVE GLASSES, SPY-GLASSES, OPERA-GLASSES. A particular kind, which is thought to be much brighter than the rest, is called a NIGHT-GLASS.

To what has commonly been stated, respecting the inventor of this most noble and useful instrument, we may add the two following claims.

Mr Leonard Digges, a gentleman of the last century, of great and various knowledge, positively asserts in his *Stratoticos*, and in another work, that his father, a military gentleman, had an instrument which he used in the field, by which he could bring distant objects near, and could know a man at the distance of three miles. He says, that when his father was at home he had often looked through it, and could distinguish the waving of the trees on the opposite side of the Severn. Mr Digges resided in the neighbourhood of Bristol.

Francis Fontana, in his *Celestial Observations*, published at Naples in 1646, says, that he was assured by a Mr Hardy, advocate of the parliament of Paris, a person of great learning and undoubted integrity, that on the death of his father, there was found among his things an old tube, by which distant objects were distinctly seen; and that it was of a date long prior to the telescope lately invented, and had been kept by him as a secret.

It is not at all improbable, that curious people handling spectacle-glasses, of which there were by this time great varieties, both convex and concave, and amusing themselves with their magnifying power and the singular effects which they produced in the appearances of things, might sometimes chance so to place them as to produce distinct and enlarged vision. We know perfectly, from the table and scheme which Sirturus has given us of the tools or dishes in which the spectacle-makers fashioned their glasses, that they had convex lenses formed to spheres of 24 inches diameter, and of 11 inferior sizes. He has given us a scheme of a set which he got leave to measure belonging to a spectacle-maker of the name of *Rogette* at Corunna in Spain; and he says that this man had tools of the same sizes for concave glasses. It also appears, that it was a general practice (of which we do not know the precise purpose) to use a convex and concave glass together. If any person should chance to put together a 24-inch convex and a 12-inch concave (wrought on both sides) at the distance of 6 inches, he would have distinct vision, and the object would appear of double size. Concaves of 6 inches were not uncommon, and one such combined with the convex of 24, at the distance of 9 inches, would have distinct vision, and objects would be quadrupled in diameter. When such a thing occurred, it was natural to keep it as a curiosity, although the *rationale* of its operation was not in the least understood. We doubt not but that this happened much oftener than in these two instances. The chief wonder is, that it was not frequent, and taken notice of by some writer. It is pretty plain that Galileo's first telescope was of this kind, made up of such spectacle-glasses as he could procure; for it magnified only three times in diameter,—a thing easily procured by such glasses as he could find with every spectacle-maker. And he could not but observe, in his trials of their glasses, that the deeper concaves and flatter convexes he employed, he produced the greater amplification; and then he would find himself obliged to provide a tool not used by the spectacle-makers, viz. either a much flatter tool for a convex surface, or a much smaller sphere for a concave: and, notwithstanding his telling us that it was by reflecting on the nature of refraction, and without any instruction, we are persuaded that he proceeded in this very way. His next telescope magnified but five times. Now the slightest acquaintance with the obvious laws of refraction would have directed him at once to a very small and deep concave, which would have been much easier made, and have magnified more. But he groped his way with such spectacle-glasses as he could get, till he at last made tools for very flat object-glasses and very deep eye-glasses, and produced a telescope which magnified about 25 times. Sirturus saw it, and took the measures of it. He afterwards saw a scheme of it which Galileo had sent to a German prince at Inspruch, who had it drawn (that is, the circles for the tools) on a table in his gallery. The object-glass was a plano-convex, a portion of a sphere, of 24 inches diameter; the eye-glass was a double concave of two inches diameter: the focal distances were therefore 24 inches and one inch nearly. This must have been a very lucky operation, for Sirturus says it was the best telescope he had seen; and we know that it requires the very best work to produce this magnifying power with such small spheres. Telescopes continued to be made in this way for many years; and Galileo, though keenly engaged in the observation of Jupiter's satellites, being candidate for the prize held out by the Dutch for the discovery of the longitude, and therefore much interested in the advantage which a convex eye-glass would have given him, never made them of any other form. Kepler published his Dioptrics in 1611; in which he tells us all that he or others had discovered of the law of refraction, viz. that in very small obliquities of incidence, the angle of refraction was nearly ⅓d of the angle of incidence. This was indeed enough to have pointed out, with sufficient exactness, the construction of every optical instrument that we are even now possessed of; for this proportionality of the angles of incidence and refraction is assumed in the construction of the optical figure for all of them; and the deviation from it is still considered as the *refinement* of the art, and was not brought to any rule till 50 years after by Huyghens, and called by him ABERRATION. Yet even the sagacious Kepler seems not to have seen the advantage of any other construction of the telescope; he just seems to acknowledge the possibility of it; and we are surprised to see writers giving him as the author of the astronomical telescope, or even as hinting at its construction. It is true, in the last proposition he shows how a telescope may be made *apparently* with a convex eye-glass: but this is only a frivolous fancy; for the eye-glass is directed to be made convex externally, and a very deep concave on the inside; so that it is, in fact, a meniscus with the concavity prevalent. In the 86th proposition, he indeed shows that it is possible so to place a convex glass behind another convex glass, that an eye shall see objects distinct, magnified, and inverted; and he speaks very sagaciously on the subject. After having said that an eye placed behind the point of union of the first glass will see an object inverted, he shows that a small part only will be seen; and then he shews that a convex glass, duly proportioned and properly placed, will show more of it. But in shewing this, he speaks in a way which shows evidently that he had formed no distinct notions of the manner in which this effect would be produced, only saying vaguely that the convergency of the second glass would counteract the divergency beyond the focus of the first. Had he conceived the matter with any tolerable distinctness, after seeing the great advantage of taking in a field greater in almost any proportion, he would have eagerly catched at the thought, and enlarged on the immense improvement. Had he but drawn one figure of the progress of the rays through two convex glasses, the whole would have been open to his view.

This step, so easy and so important, was reserved for Father Scheiner, and the construction of this author, together with that of Jansen, are the models on which all refracting telescopes are now constructed; and in all that relates to their magnifying power, brightness, and field of vision, they may be constructed on Kepler's principle, that the angles of refraction are in a certain given proportion to the angles of incidence.

But after Huyghens had applied his elegant geometry to the discovery of Snellius, viz. the proportionality, not of the angles, but of the sines, and had ascertained the aberrations from the foci of infinitely slender pencils, the reasons were clearly pointed out why there were such narrow limits affixed by nature to the performance of optical instruments, in consequence of the indistinctness of vision which resulted from constructions where the magnifying power, the quantity of light, or the field of vision, were extended beyond certain moderate bounds. The theory of aberrations, which that most excellent geometer established, has enabled us to diminish this indistinctness arising from any of these causes; and this diminution is the sole aim of all the different constructions which have been contrived since the days of Galileo and Scheiner.

The description which is commonly given of the various constructions of telescopes, is sufficient for instructing the reader in the general principles of their construction, and, with moderate attention, will show the manner in which the rays of light proceed, in order to ensure the different circumstances of amplification, brightness, and extent of field, and even distinctness of vision, in as far as this depends on the proper intervals between the glasses. But it is insufficient for giving us a knowledge of the improvements which are aimed at in the different departures from the original constructions of Galileo and Scheiner, the advantage of the double eye-glass of Huyghens, and the quintuple eye-glass of Dollond: still more is it insufficient for shewing us why the highest degrees of amplification and most extensive field cannot be obtained by the mere proportion of the focal distances of the glasses, as Kepler had taught. In short, without the Huyghenian doctrine of aberrations, neither can the curious reader learn the limits of their performance, nor the artist learn why one telescope is better than another, or in what manner to proceed to make a telescope differing in any particular from those which he servilely copies.

Although all the improvements in the construction of telescopes, since the publication of Huyghens's Dioptrics, have been the productions of this island, and although Dr Smith of Cambridge has given the most elegant and perspicuous account of this science that has yet appeared, we do not recollect a performance in the English language (except the Optics of Emerson) which will carry the reader beyond the mere schoolboy elements of the science, or enable a person of mathematical skill to understand or improve the construction of optical instruments.

We think, therefore, that we shall do the public some service, by giving such an account of this *higher branch* of optical science as will at least tend to the complete understanding of this noble instrument, by which our conceptions of the extent of almighty power, and wisdom, and beneficence, are so wonderfully enlarged. In the prosecution of this, we hope that many general rules will emerge, by which artists, who are not mathematicians, may be enabled to construct optical instruments with intelligence, and avoid the many blunders and defects which result from mere servile imitation.

The general aim in the construction of a telescope, is to form, by means of mirrors or lenses, an image of the distant object, as large, as bright, and as extensive as is possible, consistently with distinctness; and then to view the image with a magnifying glass in any convenient manner. This gives us an arrangement of our subject. We shall first show the principles of construction of the object-glass or mirror, so as that it shall form an image of the distant object with these qualities; and then show how to construct the magnifying glass or eye-piece, so as to preserve them unimpaired.

This indistinctness, which we wish to avoid, arises from two causes; the spherical figures of the refracting and reflecting surfaces, and the different refrangibility of the differently coloured rays of light. The first may be called the SPHERICAL, and the second the CHROMATIC indistinctness; and the deviations from the foci, determined by the elementary theorem, may be called the SPHERICAL and the CHROMATIC aberrations.

The limits of a work like this will not permit us to give any more of the doctrine of aberrations than is absolutely necessary for the construction of achromatic telescopes; and we must refer the reader, for a general view of the whole, to Euler's *Dioptrics*, and other works of that kind. Dr Smith has given as much as was necessary for the comparison of the merits of different glasses of similar construction, and this in a very plain and elegant manner.

We shall begin with the aberration of colour, because it is the most simple.

Let white or compounded light fall perpendicularly on the flat side P Q (Plate VI. Fig. 1.) of a plano-convex lens P V Q, whose axis is C V, and vertex V. The white ray *p* P falling on the extremity of the lens is dispersed by refraction at the point P of the spherical surface, and the red ray goes to the point *r* of the axis, and the violet ray to the point *v*. In like manner, the white ray *q* Q is dispersed by refraction at Q, the red ray going to r, and the violet to v. The red ray Pr crosses the violet ray Qv in a point D, and Qr crosses Pv in a point E; and the whole light refracted and dispersed by the circumference, whose diameter is P Q, passes through the circular area, whose diameter is D E. Supposing that the lens is of such a form that it would collect red rays, refracted by its whole surface in the point r, and violet in the point v; then it is evident, that the whole light which occupies the surface of the lens will pass through this little circle, whose diameter is D E. Therefore white light, issuing from a point so distant that the rays may be considered as parallel, will not be collected in another point or focus, but will be dispersed over the surface of that little circle, which is therefore called the *circle of chromatic dispersion*; and the radiant point will be represented by this circle. The neighbouring points are, in like manner, represented by circles; and these circles, encroaching on and mixing with each other, must occasion haziness or confusion, and render the picture indistinct. This indistinctness will be greater in the proportion of the number of circles which are in this manner mixed together. This will be in the proportion of the room that is for them; that is, in proportion to the area of the circle, or in the duplicate proportion of its diameter. Our first business, therefore, is to obtain measures of this diameter, and to mark the connexion between it and the aperture and focal distance of the lens.

Let i be to r as the sine of incidence in glass to the sine of refraction of the red rays; and let i be to v as the sine of incidence to the sine of refraction of the violet rays Then we say, that when the aperture P Q is moderate, $v - r : v + r - 2i = \mathrm{DE} : \mathrm{PQ}$, very nearly. For let D E, which is evidently perpendicular to Vr, meet the parallel incident rays in K and L, and the radii of the spherical surface in G and H. It is plain that G P K is equal to the angle of incidence on the posterior or spherical surface of the lens; and GPr and GPv are the angles of refraction of the red and the violet rays; and that GK, GD, and GE, are very nearly as the sines of those angles, because the angles are supposed to be small. We may therefore institute this proportion $DE : KD = v - r : r - i$; then, by doubling the consequents $DE : 2KD = v - r : 2r - 2i$. Also, $DE : 2KD + DE = v - r : 2r - 2i + v - r, = v - r : r + v - 2i$. But $2KD + DE$ is equal to KL or PQ. Therefore we have $DE : PQ = v - r : r + v - 2i$. *Q. E. D.*

Cor. 1. Sir Isaac Newton, by most accurate observation, found, that, in common glass, the sines of refraction of the red and violet rays were 77 and 78 where the sine of incidence was 50. Hence it follows, that $v - r$ is to $v + r - 2i$ as 1 to 55; and that the diameter of the smallest circle of dispersion is $\frac{1}{55}$th part of that of the lens.

2. In like manner may be determined the circle of dispersion that will comprehend the rays of any particular colour or set of colours. Thus all the orange and yellow will pass through a circle whose diameter is $\frac{1}{250}$th of that of the lens.

3. In different surfaces, or plano-convex lenses, the angles of aberration rPv are as the breadth PQ directly, and as the focal distance VF inversely; because any angle DPE is as its subtense DE directly, and radius DP inversely. *N. B.* We call VF the focal distance, because at this distance, or at the point F, the light is most of all constipated. If we examine the focal distance by holding the lens to the sun, we judge it to be where the light is drawn into the smallest spot.

When we reflect that a lens of 5½ inches in diameter has a circle of dispersion $\frac{1}{10}$th of an inch in diameter, we are surprised that it produces any picture of an object that can be distinguished. We should not expect greater distinctness from such a lens than would be produced in a camera obscura without a lens, by simply admitting the light through a hole of $\frac{1}{10}$th of an inch in diameter. This, we know, would be very hazy and confused. But when we remark the superior vivacity of the yellow and orange light in comparison with the rest, we may believe, that the effect produced by the confusion of the other colours will be much less sensible. But a stronger reason is, that the light is much denser in the middle of the circle of dispersion, and is exceedingly faint towards the margin. This, however, must not be taken for granted; and we must know distinctly the manner in which the light of different colours is distributed over the circle of chromatic dispersion, before we pretend to pronounce on the immense difference between the indistinctness arising from colour, and that arising from the spherical figure. We think this the more necessary, because the illustrious discoverer of the chromatic aberration has made a great mistake in the comparison, because he did not consider the distribution of the light in the circle of spherical dispersion. It is therefore proper to investigate the chromatic distribution of the light, and we shall then see that the superiority of the reflecting telescope is incomparably less than Newton imagined it to be.

Therefore, let E B (Fig. 2.) represent a plano-convex lens, of which C is the centre, and C *r* the axis. Let us suppose it to have no spherical aberration, but to collect rays occupying its whole surface to single points in the axis. Let a beam of white or compounded light fall perpendicularly on its plane surface. The rays will be so refracted by its curved surface, that the extreme red rays will be collected at *r*, the extreme violet rays at *w*, and those of intermediate refrangibility at intermediate points, *o*, *y*, *g*, *b*, *p*, *v*, of the line *r w*, which is nearly $\frac{1}{28}$th of *r* C. The extreme red and violet rays will cross each other at A and D; and A D will be a section or diameter of the circle of chromatic dispersion, and will be about $\frac{1}{55}$th of E B. We may suppose *w r* to be bisected in *b*, because *w b* is to $b\,r$ very nearly in the ratio of equality (for $r\,b : r\,\mathrm{C} = b\,\mathrm{A} : c\,\mathrm{E}, = b\,\mathrm{A} : c\,\mathrm{B}, = w\,b : w\,\mathrm{C}$.) The line $r\,w$ will be a kind of prismatic spectrum, red from r to o, orange-coloured from o to y, yellow from y to g, green from g to b, blue from b to p, purple from p to v, and violet from v to w.

The light, in its compound state, must be supposed uniformly dense as it falls upon the lens; and the same must be said of the rays of any particular colour. Newton supposes, also, that when a white ray, such as $c\,\mathrm{E}$, is dispersed into its component coloured rays by refraction at E, it is uniformly spread over the angle DEA. This supposition is indeed gratuitous; but we have no argument to the contrary, and may therefore consider it as just. The consequence is, that each point w, v, p, b, &c. of the spectrum is not only equally luminous, but also illuminates uniformly its corresponding portion of AD; that is to say, the coating (so to term it) of any particular colour, such as purple, from the point p, is uniformly dense in every part of AD on which it falls. In like manner, the colouring of yellow, intercepted by a part of AD in its passage to the point y, is uniformly dense in all its parts. But the density of the different colours in AD is extremely different; for since the radiation in w is equally dense with that in p, the density of the violet colouring, which radiates from w, and is spread over the whole of AD, must be much less than the density of the purple colouring, which radiates from p, and occupies only a part of AD round the circle b. These densities must be very nearly in the inverse proportion of $w\,b^2$ to $p\,b^2$.

Hence we see, that the central point b will be very intensely illuminated by the blue radiating from $p\,b$ and the green intercepted from $b\,g$. It will be more faintly illuminated by the purple radiating from $v\,p$, and the yellow intercepted from $g\,y$; and still more faintly by the violet from $w\,v$, and the orange and red intercepted from $y\,r$. The whole colouring will be a white, tending a little to yellowness. The accurate proportion of these colourings may be computed from our knowledge of the position of the points *o*, *y*, *g*, &c. But this is of little moment. It is of more consequence to be able to determine the proportion of the total intensity of the light in *b* to its intensity in any other point I.

For this purpose draw *r* I R, I *w* W, meeting the lens in R and W. The point I receives none of the light which passes through the space R W : for it is evident that *b* I : C R = *b* A : C E, = 1 : 55, and that C R = C W; and therefore, since all the light incident on E B passes through A B, all the light incident on R W passes through I *i* (*b i* being made = *b* I). Draw *o* I O, *y* I Y, *g* I G, I *p* P, I *v* V. It is plain that I receives red light from R O, orange from O Y, yellow from Y G, green from G E, a little blue from B P, purple from P V, and violet from V W. It therefore wants some of the green and of the blue.

That we may judge of the intensity of these colours at I, suppose the lens covered with paper pierced with a small hole at G. The green light only will pass through I; the other colours will pass between I and *b*, or between I and A, according as they are more or less refrangible than the particular green at I. This particular colour converges to *g*, and therefore will illuminate a small spot round I, where it will be as much denser than it is at G as this spot is smaller than the hole at G. The natural density at G, therefore, will be to the increased density at I, as $g\,I^2$ to $g\,G^2$, or as $g\,b^2$ to $g\,C^2$, or as $b\,I^2$ to $C\,G^2$. In like manner, the natural density of the purple coming to I through an equal hole at P will be to the increased density at I as $b\,I^2$ to $C\,P^2$. And thus it appears, that the intensity of the differently coloured illuminations of *any* point of the circle of dispersion, is inversely proportional to the square of the distance from the centre of the lens to the point of its surface through which the colouring light comes to this point of the circle of dispersion. This circumstance will give us a very easy, and, we think, an elegant solution of the question.

Bisect C E in F, and draw F L perpendicular to C E, making it equal to C F. Through the point L describe the hyperbola K L N of the second order, that is, having the ordinates E K, F L, R N, &c. inversely proportional to the squares of the abscissæ C E, C F, C R, &c.; so that F L : R N $= \frac{1}{CF^2} : \frac{1}{CR^2}$, or $= CR^2 : CF^2$, &c. It is evident that these ordinates are proportional to the densities of the severally coloured lights which go from them to any points whatever of the circle of dispersion.

Now the total density of the light at I depends both on the density of each particular colour and on the number of colours which fall on it. The ordinates of this hyperbola determine the first; and the space E R measures the number of colours which fall on I, because it receives light from the whole of E R, and of its equal B W. Therefore, if ordinates be drawn from any point of E R, their sum will be as the whole light which goes to I; that is, the total density of the light at I will be proportional to the area N R E K. Now it is known that C E × E K is equal to the infinitely extended area lying beyond E K; and C R × R N is equal to the infinitely extended area lying beyond R N. Therefore the area N R E K is equal to C R × R N —C E × E K. But R N and E K are respectively equal to $\frac{CF^3}{CR^2}$ and $\frac{CF^3}{CE^2}$. Therefore the density at I is proportional to $CF^3 \times \left(\frac{CR}{CR^2} - \frac{CE}{CE^2}\right)$, $= CF^3 \times \left(\frac{1}{CR} - \frac{1}{CE}\right)$, $= CF^3 \times \frac{CE - CR}{CE \times CR} = CF^3 \times \frac{ER}{CE \times CR}$, $= \frac{CF^3}{CE} \times \frac{ER}{CR}$. But because C F is ½ of C E, $\frac{CF^3}{CE}$ is $= \frac{CF^3}{2CF}$, =

$\frac{CF^2}{2}$, a constant quantity. Therefore the density of the light at I is proportional to $\frac{ER}{CR}$, or to $\frac{AI}{bI}$, because the points R and I are similarly situated in E C and A b.

Farther, if the semi-aperture C E of the lens be called 1, $\frac{CF^2}{2}$ is $= \frac{1}{8}$, and the density at I is $= \frac{AI}{8bI}$.

Here it is proper to observe, that since the point R has the same situation in the diameter E B that the point I has in the diameter A D of the circle of dispersion, the circle described on E B may be conceived as the magnified representation of the circle of dispersion. The point F, for instance, represents the point f in the circle of dispersion, which bisects the radius b A; and f receives no light from any part of the lens which is nearer the centre than F, being illuminated only by the light which comes through E F and its opposite B F'. The same may be said of every other point.

In like manner, the density of the light in f, the middle between b and A, is measured by $\frac{EF}{CF}$, which is $= \frac{EF}{EF}$, or 1. This makes the density at this point a proper standard of comparison. The density there is to the density at I as 1 to $\frac{AI}{bI}$, or as b I to A I; and this is the simplest mode of comparison. The density half way from the centre of the circle of dispersion is to the density at any point I as b I to I A.

Lastly, through L describe the common rectangular hyperbola k L n, meeting the ordinates of the former in k, L, and n; and draw k h parallel to E C, cutting the ordinates in g, f, r, &c. Then C R : C E = E k : R n, and C R : C E—C R = E k : R n—E k, or C R : R E = E k : rn, and b I : I A = E k : r n. And thus we have a very simple expression of the density in any point of the circle of dispersion. Let the point be any where, as at I. Divide the lens at R as A D is divided in I, and then $r\,n$ is as the density in I.

These two measures were given by Newton; the first in his Treatise *de Mundi Systémate*, and the last in his *Optics*; but both without demonstration.

If the hyperbola k L n be made to revolve round the axis C Q, it will generate a solid spindle, which will measure the whole quantity of light which passes through different portions of the circle of dispersion. Thus the solid produced by the revolution of L $k\,f$ will measure all the light which occupies the outer part of the circle of dispersion lying without the middle of the radius. This space is ¾ths of the whole circle; but the quantity of light is but ¼th of the whole.

A still more simple expression of the whole quantity of light passing through different portions of the circle of chromatic dispersion may now be obtained as follows:

It has been demonstrated, that the density of the light at I is as $\frac{\mathrm{A\,I}}{b\,\mathrm{I}}$, or as $\frac{\mathrm{E\,R}}{\mathrm{C\,R}}$. Suppose the figure to turn round the axis. I or R describe circumferences of circles; and the whole light passing through this circumference is as the circumference or as the radius, and as the density jointly. It is therefore as $\frac{\mathrm{E\,R}}{\mathrm{C\,R}} \times \mathrm{C\,R}$, that is, as E R. Draw any straight line E m, cutting R N in s, and any other ordinate F L in x R s. The whole light which illuminates the circumference described by I is to the whole light which illuminates the centre b as E R to E C, or as R s to C m. In like manner, the whole light which illuminates the circumference described by the point f in the circle of dispersion is to the whole light which illuminates the centre b, as F x to C m. The lines C m, R S, F x, are therefore proportional to the whole light which illuminates the corresponding circumferences in the circle of dispersion.

Therefore the whole light which falls on the circle whose radius is bI, will be represented by the trapezium in CRS; and the whole light which falls on the ring described by I A, will be represented by the triangle E s R; and so of any other portions.

By considering the figure, we see that the distribution of the light is exceedingly unequal. Round the margin it has no sensible density; while its density in the very centre is incomparably greater than in any other point, being expressed by the asymptote of a hyperbola. Also the circle described with the radius $\frac{Ab}{2}$ contains ¾ths of the whole light. No wonder then that the confusion caused by the mixture of these circles of dispersion is less than one should expect; besides, it is evident that the most lively or impressive colours occupy the middle of the spectrum, and are there much denser than the rest. The margin is covered with an illumination of deep red and violet, neither of which colours are brilliant. The margin will be of a dark claret colour. The centre revives all the colours, but in a proportion of intensity greatly different from that in the common prismatic spectrum, because the radiant points L, p, b, g, &c. by which it is illuminated, are at such different distances from it. It will be white; but we apprehend not a pure white, being greatly overcharged with the middle colours.

These considerations show that the coloured fringes, which are observed to border very luminous objects seen on a dark ground through optical instruments, do not proceed from the object-glass of a telescope or microscope, but from an improper construction of the eye-glasses. The chromatic dispersion would produce fringes of a different colour, when they produce any at all, and the colours would be differently disposed. But this dispersion by the object-glass can hardly produce any fringes: its effect is a general and almost uniform mixture of circles all over the field, which produces an uniform haziness, as if the object were viewed at an improper distance, or out of its focus, as we vulgarly express it.

We may at present form a good guess at the limit which this cause puts to the performance of a telescope. A point of a very distant object is represented, in the picture formed by the object-glass, by a little circle, whose diameter is at least $\frac{1}{250}$th of the aperture of the object-glass, making a very full allowance for the superior brilliancy and density of the central light. We look at this picture with a magnifying eye-glass. This magnifies the picture of the point. If it amplify it to such a degree as to make it an object individually distinguishable, the confusion is then sensible. Now this can be computed. An object subtending one minute of a degree is distinguished by the dullest eye, even although it be a dark object on a bright ground. Let us therefore suppose a telescope, the object-glass of which is of six feet focal distance, and one inch aperture. The diameter of the circle of chromatic dispersion will be $\frac{1}{300}$th of an inch, which subtends at the centre of the object-glass an angle of about 9½ seconds. This, when magnified six times by an eye-glass, would become a distinguishable object; and a telescope of this length would be indistinct if it magnified more than six times, if a point were thus spread out into a spot of uniform intensity. But the spot is much less intense about its margin. It is found experimentally that a piece of engraving, having fine cross hatches, is not sensibly indistinct till brought so far from the limits of perfectly distinct vision, that this indistinctness amounts to 6′ or 5′ in breadth.—Therefore such a telescope will be sensibly distinct when it magnifies 36 times; and this is very agreeable to experience.

We come, in the second place, to the more arduous task of ascertaining the error arising from the spherical figure of the surfaces employed in optical instruments.—Suffice it to say, before we begin, that although geometers have exhibited other forms of lenses which are totally exempt from this error, they cannot be executed by the artist; and we are therefore restricted to the employment of spherical surfaces.

Of all the determinations which have been given of spherical aberration, that by Dr Smith, in his Optics, which is an improvement of the fundamental theorem of that most elegant geometer Huyghens, is the most perspicuous and palpable. Some others are more concise, and much better fitted for after use, and will therefore be employed by us in the prosecution of this article. But they do not keep in view the optical facts, giving the mind a picture of the progress of the rays, which it can contemplate and discover amidst many modifying circumstances. By ingenious substitutions of analytical symbols, the investigation is rendered expeditious, concise, and certain; but these are not immediate symbols of things, but of operations of the mind; objects sufficiently subtile of themselves, and having no need of substitutions to make us lose sight of the real subject; and thus our occupation degenerates into a process almost without ideas. We shall therefore set out with Dr Smith's fundamental Theorem.

1. *In Reflections.*

Let A B V (Fig. 3.) be a concave spherical mirror, of which C is the centre, V the vertex, C V the axis, and F the focus of an infinitely slender pencil of parallel rays passing through the centre. Let the ray *a* A, parallel to the axis, be reflected in A G, crossing the central ray C V in *f*. Let A P be the sine of the semi-aperture A V, A D its tangent, and CD its secant.

The aberration F *f* from the principal focus of central rays is equal to $\frac{1}{2}$ of the excess V D of the secant above the radius, or very near equal to $\frac{1}{4}$ of V P, the versed sine of the semi-aperture.

For because A D is perpendicular to C A, the points C, A, D, are in a circle, of which C D is the diameter; and because A *f* is equal to C *f*, by reason of the equality of the angles *f* A C, *f* C A, and C A *a*, *f* is the centre of the circle through C, A, D, and *f* D is $= \frac{1}{2}$ C D. But F C is $= \frac{1}{2}$ C V. Therefore F *f* is $\frac{1}{2}$ of V D.

But because D V : V P = D C : V C, and D C is very little greater than V C when the aperture A B is moderate, D V is very little greater than V P, and F *f* is very nearly equal to $\frac{1}{2}$ of V P.

Cor. 1. The longitudinal aberration is $= \frac{AV^2}{4CV}$, for P V is very nearly $= \frac{AV^2}{2CV}$.

Cor. 2. The lateral aberration F G is $= \frac{AV^3}{2CV^2}$. For F G : F *f* = A P : P *f*, = A V : $\frac{1}{2}$ C V nearly, and therefore $FG = \frac{AV^3}{4CV} \times \frac{2}{CV} = \frac{AV^3}{2CV^2}$.

2. *In Refractions.*

Let A V B (Fig. 4. or 5) be a spherical surface separating two refracting substances, C the centre, V the vertex, A V the semi-aperture, A P its sine, P V its versed sine, and F the focus of parallel rays infinitely near to the axis. Let the extreme ray *a* A, parallel to the axis, be refracted into A G, crossing C F in *f*, which is therefore the focus of extreme parallel rays.

The rectangle of the sine of incidence, by the difference of the sines of incidence and refraction, is to the square of the sine of refraction, as the versed sine of the semi-aperture is to the longitudinal aberration of the extreme rays.

Call the sine of incidence *i*, the sine of refraction *r*, and their difference *d*.

Join C A, and about the centre *f* describe the arch A D.

The angle ACV is equal to the angle of incidence, and CAf is the angle of refraction. Then, since the sine of incidence is to the sine of refraction as VF to CF, or as Af to Cf, that is, as Df to Cf, we have

$$\text{CF} : \text{FV} = \text{C}f : f\text{D}$$

by conversion $\text{CF} : \text{CV} = \text{C}f : \text{CD}$

altern. conver. $\text{CF} - \text{C}f : \text{CV} - \text{CD} = \text{CF} : \text{CV}$

or - - $\text{F}f : \text{VD} = \text{CF} : \text{CV}, = r : d.$

Now $\text{PV} = \frac{\text{AP}^2}{\text{CP}+\text{CV}}, = \frac{\text{AP}^2}{2\text{CV}}$ nearly, and $\text{PD} = \frac{\text{AP}^2}{f\text{P}+f\text{V}} = \frac{\text{AP}^2}{2f\text{V}}$ nearly, $= \frac{\text{AP}^2}{2\text{FV}}$ nearly. Therefore $\text{PV} : \text{PD} = \text{FV} : \text{CV}$, and $\text{DV} : \text{PV} = \text{CF} : \text{FV}$ nearly.

We had above $\text{F}f : \text{VD} = r : d;$

and now - $\text{VD} : \text{PV} = \text{CF} : \text{FV}, = r : i;$

therefore - $\text{F}f : \text{PV} = r^2 : di;$

and $\text{F}f = \frac{r^2}{di} \times \text{PV}$. *Q. E. D.*

The aberration will be different according as the refraction is made towards or from the perpendicular; that is, according as r is less or greater than i. They are in the ratio of $\frac{r^2}{di}$ to $\frac{i^2}{dr}$, or of r^3 to i^3. The aberration therefore is always much diminished when the refraction is made from a rare into a dense medium. The proportion of the sines for air and glass is nearly that of 3 to 2. When the light is refracted into the glass, the aberration is nearly $\frac{3}{4}$ of P V; and when the light passes out of glass into air, it is about $\frac{9}{2}$ of P V.

Cor. 1. $\text{F}f = \frac{r^2}{di} \times \frac{\text{AP}^2}{2\text{CV}}$ nearly, and it is also $= \frac{r^2}{d^2} \times \frac{\text{AP}^2}{2\text{FV}}$, because $\text{PV} = \frac{\text{AP}^2}{2\text{CV}}$ nearly, and $i : d = \text{FV} : \text{CV}$.

Cor. 2. Because $fP : PA = Ff : FG$
or $FV : AV = Ff : FG$ nearly,

we have F G, the lateral aberration, $= Ff \times \frac{AV}{FV}, = \frac{r^2}{d^2} \times \frac{AV^3}{2\,FV^2}, = \frac{r^2}{i^2} \times \frac{AV}{2\,CV^2}$.

Cor. 3. Because the angle F A f is proportional to $\frac{FG}{FV}$ very nearly, we have the angular abberration $FAf = \frac{r^2}{d^2} \times \frac{AV^3}{2\,FV^3} = \frac{r^2}{i^2} \times \frac{AV^3}{2\,CV^3}$.

In general, the longitudinal aberrations from the focus of central parallel rays are as the squares of the apertures directly, and as the focal distances inversely; and the lateral aberrations are as the cubes of the apertures directly, and the squares of the focal distances inversely; and the angular aberrations are as the cubes of the aperture directly, and the cubes of the focal distances inversely.

The reader must have observed, that to simplify the investigation, some small errors are admitted. PV and PD are not in the exact proportion that we assumed them, nor is Df equal to F V. But in the small apertures which suffice for optical instruments, these errors may be disregarded.

This spherical aberration produces an indistinctness of vision, in the same manner as the chromatic aberration does, viz. by spreading out every mathematical point of the object into a little spot in its picture; which spots, by mixing with each other, confuse the whole. We must now determine the diameter of the circle of diffusion, as we did in the case of chromatic dispersion.

Let the ray $\beta\alpha$ (Fig. 6) be refracted on the other side of the axis, into $\alpha H \varphi$, cutting AfG in H, and draw the perpendicular E H. Call AV a, $\alpha V\ \alpha$, Vf (or V F, or Vφ, which in this comparison may be taken as equal) $= f$, Ef $= b$, and fE $= x$.

$AV^2 : \alpha V^2 = Ff : F\phi$ (already demonstrated) and $F\phi = \frac{\alpha^2}{a^2} b$, and $Ff - F\phi$, (or $f\phi$) $= b - \frac{\alpha^2}{a^2} b, = \frac{a^2 b - \alpha^2 b}{a^2} = \frac{b}{a^2} \times a^2 - \alpha^2, = \frac{b}{a^2} \times \overline{a+\alpha} \times \overline{a-\alpha}$. Also $Pf : PA = fE : EH$, or $f : a = x : \frac{a x}{f} = EH$. And $P\pi : P\phi = EH : E\phi$, or $\alpha : f = \frac{a x}{f} : \frac{a x}{\alpha} = E\phi$. Therefore $f\phi = \frac{a x}{\alpha} + x, = \frac{\overline{a+\alpha}\, x}{\alpha}, = \frac{x}{\alpha} \times \overline{a+\alpha}$. Therefore $\frac{x}{\alpha} \times \overline{a+\alpha} = \frac{b}{a^2} \times \overline{a+\alpha} \times \overline{a-\alpha}$, and $\frac{x}{\alpha} = \frac{b}{a^2} \times \overline{a-\alpha}$, and $x = \frac{b}{a^2} \times \alpha\,(a - \alpha)$. Therefore x is greatest when $\alpha \times \overline{a-\alpha}$ is greatest; that is, when $\alpha = \frac{1}{2} a$. Therefore EH is greatest when $P\pi$ is equal to the half of AP. When this is the case, we have at the same time $\frac{b}{a^2} \times \alpha\,(a - \alpha) = \frac{b}{a^2} \times \frac{1}{4} a^2$, and $x = \frac{1}{4} b$, or $EH = \frac{1}{4} FG$. That is, the diameter of the circle of aberration through which the whole of the refracted light must pass, is $\frac{1}{4}$ of the diameter of the circle of aberration at the focus of parallel central rays. In the chromatic aberration it was $\frac{1}{2}$; so that, in this respect, the spherical aberration does not create so great confusion as the chromatic.

We are now able to compare them, since we have now the measure of both the circles of aberration.

It has not been found possible to give more than four inches of aperture to an object-glass of 100 feet focal distance, so as to preserve sufficient distinctness. If we compute the diameter of the circle EH corresponding to this aperture, we shall find it not much to exceed $\frac{1}{120{,}000}$ of an inch. If we restrict the circle of chromatic dispersion to $\frac{1}{250}$ of the aperture, which is hardly the fifth part of the whole dispersion in it, it is $\frac{1}{62\frac{1}{2}}$ of an inch, and is about 1900 times greater than the other.

The circle of spherical aberration of a plano-convex lens, with the plane side next the distant object, is equal to the circle of chromatic dispersion when the semi-aperture is about 15^0: For we saw formerly that E H is $\frac{1}{4}$ of F G, and that F G is $= \frac{r^2}{i^2} \frac{AP^3}{2AC^2}$, and therefore E G $= \frac{r^2}{i^2} \times \frac{AP^3}{8AC^2}$. This being made $= \frac{AP}{55}$, gives us A P $= \sqrt{\frac{8i^2 AC^2}{55r^2}}$, which is nearly $\frac{AC}{4}$, and corresponds to an aperture of 30^0 diameter, if r be to i as 3 to 2.

Sir Isaac Newton was therefore well entitled to say, that it was quite needless to attempt figures which should have less aberration than spherical ones, while the confusion produced by the chromatic dispersion remained uncorrected. Since the indistinctness is as the squares of the diameters of the circles of aberration, the disproportion is quite beyond our imagination, even when Newton has made such a liberal allowance to the chromatic dispersion. But it must be acknowledged, that he has not attended to the distribution of the light in the circle of spherical aberration, and has hastily supposed it to be like the distribution of the coloured light, indefinitely rare in the margin, and denser in the centre.

We are indebted to Father Boscovich for the elegant determination of this distribution, from which it appears, that the light in the margin of the circle of spherical aberration, instead of being incomparably rarer than in the spaces between it and the centre, is incomparably denser. The indistinctness therefore produced by the intersection of these luminous circumferences is vastly great, and increases the whole indistinctness exceedingly. By a gross calculation which we have made, it appears to be increased at least 500 times. The proportional indistinctness, therefore, instead of being 1900^2 to 1, is only $\frac{1900^2}{500}$, or nearly 7220 to 1—a proportion still sufficiently great to warrant Newton's preference of the reflecting telescope of his invention. And we may now observe, that the reflecting telescope has even a great advantage over a refracting one of the same focal distance, with respect to its spherical aberration: For we have seen (*Cor*. 2.) that the lateral aberration is $\frac{r^2}{i^2} \frac{AV^3}{2CV^2}$. This for a plano-convex glass is nearly $\frac{9}{4} \frac{AV^3}{2CV^2}$. And the diameter of the circle of aberration is one-fourth of this, or $\frac{9}{16} \times \frac{AV^3}{2CV^2}$. In like manner, the lateral aberration of a concave mirror is $\frac{AV^3}{2CV^2}$; and the diameter of the circle of dispersion is $\frac{AV^3}{8CV^2}$; and therefore, if the surfaces were portions of the same sphere, the diameter of the circle of aberration of refracted rays would be to that of the circle of aberration of reflected rays as $\frac{9}{16}$ to $\frac{1}{4}$ or as 9 to 4. But when the refracting and reflecting surfaces, in the position here considered, have the same focal distance, the radius of the refracting surface is four times that of the reflecting surface. The proportion of the diameters of the circles of spherical aberration is that of 9×4^2 to 4, or of 144 to 4, or 36 to 1. The distinctness, therefore, of the reflector is 36×36, or 1296 times greater than that of a plano-convex lens (placed with the plane side next the distant object) of the same breadth and focal distance, and will therefore admit of a much greater magnifying power. This comparison is indeed made in circumstances most favourable to the reflector, because this is the very worst position of a plano-convex lens. But we have not as yet learned the aberration in any other position. In another position, the refraction and consequent aberration of both surfaces are complicated.

Before we proceed to the consideration of this very difficult subject, we may deduce, from what has been already demonstrated, several general rules and maxims in the construction of telescopes, which will explain (to such readers as do not wish to enter more deeply into the subject) and justify the proportion which long practice of the best artists has sanctioned.

Indistinctness proceeds from the commixture of the circles of aberration on the retina of the eye: For any one *sensible* point of the retina, being the centre of a circle of aberration, will at once be affected by the admixture of the rays of as many different pencils of light as there are sensible points in the area of that circle, and will convey to the mind a mixed sensation of as many visible points of the object. This number will be as the area of the circle of aberrations, whatever be the size of a sensible point of the retina. Now, in vision with telescopes, the diameter of the circle of aberration on the retina is as the *apparent* magnitude of the diameter of the corresponding circle in the focus of the eye-glass; that is, as the angle subtended by this diameter at the centre of the eye-glass; that is, as the diameter itself directly, and as the focal distance of the eye-glass inversely. And the area of that circle on the retina is as the area of the circle in the focus of the eye-glass directly, and as the square of the focal distance of the eye-glass inversely. And this is the measure of the apparent indistinctness.

Cor. In all sorts of telescopes, and also in compound microscopes, an object is seen equally distinct when the focal distance of the eye-glasses are proportional to the diameters of the circles of aberration in the focus of the object-glass.

Here we do not consider the trifling alteration which well-constructed eye-glasses may add to the indistinctness of the first image.

In refracting telescopes, the apparent indistinctness is as the area of the object-glass directly, and as the square of the focal distance of the eye-glass inversely. For it has been shown, that the area of the circle of dispersion is as the area of the object-glass, and that the spherical aberration is insignificant when compared with this.

Therefore, to make reflecting telescopes equally distinct, the diameter of the object-glass must be proportional to the focal distance of the eye-glass.

But in reflecting telescopes, the indistinctness is as the sixth power of the aperture of the object-glass directly, and as the fourth power of the focal distance of the object-glass and square of the focal distance of the eye-glass inversely. This is evident from the dimensions of the circle of aberration, which was found proportional to $\frac{AV^3}{CV^2}$.

Therefore, to have them equally distinct, the cubes of the apertures must be proportional to the squares of the focal distance multiplied by the focal distance of the eye-glass.

By these rules, and a standard telescope of approved goodness, an artist can always proportion the parts of any instrument he wishes to construct. Mr Huyghens made one, of which the object-glass had 30 feet focal distance and three inches diameter. The eye-glass had 3,3 inches focal distance. And its performance was found superior to any which he had seen; nor did this appear owing to any chance goodness of the object-glass, because he found others equally good which were constructed on similar proportions. This has therefore been adopted as a standard.

It does not at first appear how there can be any difficulty in this matter, because we can always diminish the aperture of the object-glass or speculum till the circle of aberration is as small as we please. But by diminishing this aperture, we diminish the light in the duplicate ratio of the aperture. Whatever be the aperture, the brightness is diminished by the magnifying power, which spreads the light over a greater surface in the bottom of the eye. The apparent brightness must be as the square of the aperture of the telescope directly, and the square of the amplification of the diameter of an object inversely. Objects, therefore, will be seen equally bright, if the apertures of the telescopes be as the focal distances of the object-glasses directly, and the focal distance of the single eye-glass (or eye-glass equivalent to the eye-piece) inversely. Therefore, to have telescopes equally distinct and equally bright, we must combine these proportions with the former. It is needless to go farther into this subject, because the construction of refracting telescopes has been so materially changed by the correction of the chromatic aberration, that there can hardly be given any proportion between the object-glass and eye-glasses. Every thing now depends on the degree in which we can correct the aberrations of the object-glass. We have been able so far to diminish the chromatic aberration, that we can give very great apertures without its becoming sensible. But this is attended with so great an increase of the aberration of figure, that this last becomes a sensible quality. A lens which has 30° for its semi-aperture, has a circle of aberration equal to its chromatic aberration. Fortunately we can derive from the very method of contrary refractions, which we employ for removing the chromatic aberration, a correction of the other. We are indebted for this contrivance, also, to the illustrious Newton.

We call this Newton's contrivance, because he was the first who proposed a construction of an object-glass, in which the aberration was corrected by the contrary aberrations of glass and water.

Huyghens had indeed supposed, that our all-wise Creator had employed in the eyes of animals many refractions in place of one, in order to make the vision more distinct; and the invidious detractors from Newton's fame have catched at this vague conjecture as an indication of his knowledge of the possibility of destroying the aberration of figure by contrary refractions. But this is very ill-founded. Huyghens has acquired sufficient reputation by his theory of aberrations. The scope of his writing in the passage alluded to, is to show, that by dividing any intended refraction into parts, and producing a certain convergence to or divergence from the axis of an optical instrument by means of two or three lenses instead of one, we diminish the aberrations four or nine times. This conjecture about the eye was therefore in the natural train of his thoughts. But he did not think of destroying the aberration altogether by opposite refractions. Newton, in 1669, says, that opticians need not trouble themselves about giving figures to their glasses other than spherical. If this figure were all the obstacle to the improvement of telescopes, he could shew them a construction of an object-glass, having spherical surfaces, where the aberration is destroyed; and accordingly gives the construction of one composed of glass and water, in which this is done completely by means of contrary refractions.

The general principle is this: When the radiant point R, (Fig. 7.) or focus of incident rays, and its conjugate focus F of refracted central rays, are on opposite sides of the refracting surface or lens V, the conjugate focus f of marginal rays is nearer to R than F is. But when the focus of incident rays R′ lies on the same side with its conjugate focus F′ for central rays, R′f' is greater than R′ F′.

Now Fig. 8. represents the contrivance for destroying the colour produced at F, the principal focus of the convex lens V, of crown glass, by means of the contrary refraction of the concave lens v of flint glass. The incident parallel rays are made to converge to F by the first lens. This convergence is diminished, but not entirely destroyed, by the concave lens v, and the focus is formed in F. F and F′ therefore are conjugate foci of the concave lens. If F be the focus of V for central rays, the marginal rays will be collected at some point f nearer to the lens. If F be now considered as the focus of light incident on the centre of v, and F′ be the conjugate focus, the marginal ray p F would be refracted to some point f' lying beyond F′. Therefore the marginal ray pf may be refracted to F′, if the aberration of the concave be properly adjusted to that of the convex.

This brings us to the most difficult part of our subject, the compounded aberrations of different surfaces. Our limits will not give us room for treating this in the same elementary and perspicuous manner that we employed for a single surface. We must try to do it in a compendious way, which will admit at once the different surfaces and the different refractive powers of different substances. This must naturally render the process more complicated; but we hope to treat the subject in a way easily comprehended by any person moderately acquainted with common algebra; and we trust that our attempt will be favourably received by an indulgent public, as it is (as far as we know) the only dissertation in our language on the construction of achromatic instruments. We cannot but express our surprise at this indifference about an invention which has done so much honour to our country, and which now constitutes a very lucrative branch of its manufacture. Our artists infinitely surpass all the performances of foreigners in this branch, and supply the markets of Europe without any competition; yet it is from the writings on the Continent that they derive their scientific instruction, and particularly from the dissertations of Clairaut, who has wonderfully simplified the analysis of optical propositions. We shall freely borrow from him, and from the writings of Abbé Boscovich, who has considerably improved the first views of Clairaut. We recommend the originals to the curious reader. Clairaut's dissertations are to be found in the Memoirs of the Academy of Paris, 1756, &c.; those of Boscovich in the Memoirs of the Academy of Bologna, and in his five volumes of *Opuscula*, published at Bassano in 1785. To these may be added D'Alembert and Euler.

Lemma 1. In the right-angled triangle MXS, (Fig. 9.) of which one side MX is very small in comparison of either of the others; the excess of the hypothenuse MS, above the side XS, is very nearly equal to $\frac{MX^2}{2MS}$, or to $\frac{MX^2}{2XS}$. For if about the centre S, with the radius SM, we describe the semicircle AMO, we have $AX \times XO = MX^2$. Now $AX = MS - SX$, and XO, is nearly equal to 2MS or 2XS; on the other hand, MS is nearly equal to $XS + \frac{MX^2}{2XS}$; and, in like manner, MG is nearly equal to $\frac{MX^2}{2XG} + XG$, and MH is nearly equal to $\frac{MX^2}{2XH} + XH$.

Prop. I. Let the ray mM, incident on the spherical surface AM, converge to G; that is, let G be the focus of incident rays. It is required to find the focus F of refracted rays?

Let m express the ratio of the sine of incidence and refraction; that is, let m be to 1 as the sine of incidence to the sine of refraction in the substance of the sphere.

Then $\quad MG : GS = \sin. MSH : \sin. SMG$,
and $\quad m : 1 = \sin. SMG : \sin. SMH$;

therefore $m \times MG : GS = \sin. MSH : \sin. SMH$. Now $S, MSH : S, SMH = MH : HS$. Therefore, finally, $m. MG : GS = MH : HS$.

Now let M S, the radius of the refracting surface, be called a. Let A G, the distance of the focus of incident rays from the surface, be called r. And let A H, the focal distance of refracted rays, be called x. Lastly, let the sine M X of the semi-aperture be called e. Observe, too, that a, r, x, are to be considered as positive quantities, when A S, A G, A H, lie from the surface in the direction in which the light is supposed to move. If, therefore, the refracting surface be concave, that is, having the centre on that side from which the light comes; or if the incident rays are divergent, or the refracted rays are divergent; then a, r, x, are negative quantities.

It is plain that $HS = x - a$; $GS = r - a$; also $AX = \frac{e^2}{2a}$ nearly. $HX = a - \frac{e^2}{2a}$. $GX = r - \frac{e^2}{2a}$. Now add to H X and to G X their differences from M H and M G, which (by the Lemma) are $\frac{e^2}{2x}$ and $\frac{e^2}{2r}$. We get $MH = x - \frac{e^2}{2a} + \frac{e^2}{2x}$, and $MG = r - \frac{e^2}{2a} + \frac{e^2}{2r}$. In order to shorten our notation, make $k = \frac{1}{a} - \frac{1}{r}$. This will make $MG = r - \frac{ke^2}{2}$.

Now substitute these values in the final analogy at the top of this column, viz. $MH : HS = m. MG : GS$; it becomes $x - \frac{e^2}{2a} + \frac{e^2}{2x} : x - a = mr - \frac{mke^2}{2} : r - a$ (or ark), because $k = \frac{r-a}{ar}$, and $ark = r - a$. Now multiply the extreme and mean terms of this analogy. It is evident that it must give us an equation which will give us a value of x or A H, the quantity sought.

But this equation is quadratic. We may avoid the solution by an approximation which is sufficiently accurate, by substituting for x in the fraction $\frac{e^2}{2x}$ (which is very small in all cases of optical instruments), an approximate value very easily obtained, and very near the truth. This is the focal distance of an infinitely slender pencil of rays converging to G. This we know by the common optical theorem to be $\frac{amr}{\overline{m-1}\,r \pm a}$. Let this be called ϕ; if we substitute k in place of $\frac{1}{a}-\frac{1}{r}$, this value of ϕ becomes $=\frac{am}{m-ak}$.

This gives us, by the bye, an easily remembered expression (and beautifully simple) of the refracted focus of an infinitely slender pencil, corresponding to any distance r of the radiant point. For since $\phi=\frac{am}{m-ak}$, $\frac{1}{\phi}$ must be $=\frac{m-ak}{am}, =\frac{m}{am}-\frac{ak}{am}, =\frac{1}{a}-\frac{k}{m}$. We may even express it more simply, by expanding k, and it becomes $\frac{1}{\phi}=\frac{1}{a}-\frac{1}{ma}-\frac{1}{mr}$.

Now put this value of $\frac{1}{\phi}$ in place of the $\frac{1}{x}$ in the analogy employed above. The first term of the analogy becomes $x-\frac{e^2}{2a}+\frac{e^2}{2a}-\frac{ke^2}{2m}$, or $x-\frac{ke^2}{2m}$. The analogy now becomes $x-\frac{ke^2}{2m} : xa = mr-\frac{mke^2}{2} : ark$. Hence we obtain the linear equation $mrx-\frac{mke^2x}{2}-mra+\frac{mkae^2}{2}=arkx-\frac{arke^2}{2m}$; from which we finally deduce

$$x=\frac{mra-\frac{1}{2}make^2-\frac{ark^2e^2}{2m}}{mr-ark-\frac{1}{2}mke^2}$$

We may simplify this greatly by attending to the elementary theorem in fluxions, that the fraction $\frac{x+\dot{x}}{y+\dot{y}}$ differs from the fraction $\frac{x}{y}$ by the quantity $\frac{y\dot{x}-x\dot{y}}{y^2}$; this being the fluxion of $\frac{x}{y}$. Therefore $\frac{x+\dot{x}}{y+\dot{y}}=\frac{x}{y}+\frac{y\dot{x}-x\dot{y}}{y^2}$. Now the preceding formula is nearly in this situation. It may be written thus; $\frac{mra \quad (-\frac{1}{2}make^2-\frac{ark^2e^2}{2m})}{mr-ark \quad -mke^2}$, when the last terms of the numerator and denominator are very small in comparison with the first, and may be considered as the $\dot{x}$ and $\dot{y}$, while mra is the x, and $mr-ark$ is the y. Treating it in this way, it may be stated thus:

$$x=\frac{mra}{mr-ark}+\frac{(mra)\frac{1}{2}mke^2-(mr-ark)(\frac{1}{2}mkae^2+\frac{ark^2e^2}{2m})}{r^2(m-ak)^2}$$

$$\text{or } x=\frac{mra}{r(m-ak)}+\frac{(mra)\,mk-(mr-ark)\,(mka+\frac{ark^2}{m})}{r^2(m-ak)^2}$$

$\times\frac{1}{2}e$.

The first term $\frac{mra}{r(m-ak)}$, or $\frac{ma}{m-ak}$, is evidently $=\varphi$, the focal distance of an infinitely slender pencil. Therefore the aberration is expressed by the second term, which we must endeavour to simplify.

If we now perform the multiplications indicated by $-(mr-ark)\times(mka-\frac{ark^2}{m})$, it is plain that $-mr\times mka$ destroys the first term $mra\times mk$ of the numerator of our small fraction, and there remains of this numerator $\left(m\,a^2\,r\,k^2 - a\,r^2\,k^2 + \frac{a^2\,r^2\,k}{m}\right)\frac{1}{2}\,e^2$, which is equal to $m^2\,a^2\left(\frac{r\,k^2}{m} - \frac{r^2\,k^2}{m^2\,a} + \frac{r^2\,k^3}{m^3}\right)\frac{1}{2}\,e^2$.

The denominator was $r^2\,(m - a\,k)^2$, and the fraction now becomes $\frac{m^2\,a^2}{(m - a\,k)^2}\left(\frac{k^2}{m r} - \frac{k^2}{m^2\,a} + \frac{k^3}{m^3}\right)\frac{1}{2}e^2$, which is evidently $= \varphi^2\left(\frac{k^2}{m\,r} - \frac{k^2}{m^2\,a} + \frac{k^3}{m^3}\right)\frac{e^2}{2}$. Now recollect that $k = \frac{1}{a} - \frac{1}{r}$. Therefore $\frac{k^3}{m^2} = \frac{k^2}{m^2}\left(\frac{1}{a} - \frac{1}{r}\right) = \frac{k^2}{m^2\,a} - \frac{k^2}{m^2\,r}$. Therefore, instead of $\frac{k^2}{m^2\,a}$, write $\frac{-k^3}{m^2} - \frac{k^2}{m\;r}$, and we get the fraction $\varphi^2\left(\frac{k^3}{m^3} - \frac{k^3}{m^2} - \frac{k^2}{m^2\,r} + \frac{k^2}{mr}\right)\frac{e^2}{2} = \varphi^2\left(\frac{k^3}{m} - \frac{m\,k^3}{m^3} - \frac{m\,k^2}{m^3\,r} + \frac{m^2\,k^2}{m^3\,r}\right)\frac{e^2}{2}$, which is equal to $\varphi^2\,\frac{1 - m}{m^3}\left(k^3 - \frac{m\,k^2}{r}\right)\frac{e^2}{2}$, and finally to $-\varphi^2\,\frac{m - 1}{m^3}\left(k^3 - \frac{m\,k^2}{r}\right)\frac{e^2}{2}$.

Therefore the focal distance of refracted rays is

$$x = \varphi - \varphi^2\,\frac{m - 1}{m^3}\left(k^3 - \frac{m\,k^2}{r}\right)\frac{e^2}{2}.$$

This consists of two parts. The first φ is the focal distance of an infinitely slender pencil of central rays, and the other $-\varphi^2\,\frac{m - 1}{m^3}\left(k^3 - \frac{m\,k^2}{r}\right)\frac{e^2}{2}$ is the aberration arising from the spherical figure of the refracting surface.

Our formula has thus at last put on a very simple form, and is vastly preferable to Dr Smith's for practice.

This aberration is evidently proportional to the square of the semi-aperture, and to the square of the distance φ: but, in order to obtain this simplicity, several quantities were neglected. The assumption of the equality of A X to $\frac{e^2}{2\,a}$ is the first source of error. A much more accurate value of it would have been $\frac{2\,a\,e^2}{4\,a^2 + e^2}$, for it is really $= \frac{e^2}{2\,a - \mathrm{A\,X}}$.

If for A X we substitute its approximated value $\frac{e^2}{2a}$, we should have $\mathrm{AX} = \frac{e^2}{2a - \frac{e^2}{2a}}, = \frac{2ae^2}{4a^2 - e^2}$. To have used this value would not have much complicated the calculus; but it did not occur to us till we had finished the investigation, and it would have required the whole to be changed. The operation in pages 435 and 436 is another source of error. But these errors are very inconsiderable when the aperture is moderate. They increase for the most part with an increase of aperture, but not in the proportion of any regular function of it; so that we cannot improve the formula by any manageable process, and must be contented with it. The errors are precisely the same with those of Dr Smith's theorem, and indeed with those of any that we have seen, which are not vastly more complicated.

As this is to be frequently combined with subsequent operations, we shorten the expression by putting ι for $\frac{m-1}{m^3}\left(k^3 - \frac{mk^2}{r}\right)\frac{e^2}{2}$. Then $\varphi^2\iota$ will express the aberration of the first refraction from the focal distance of an infinitely slender pencil; and now the focal distance of refracted rays is $f = \varphi - \varphi^2\iota$.

If the incident rays are parallel, r becomes infinite, and $\iota = \frac{m-1}{m^3}k^3\frac{e^2}{2}$. But, in this case, k becomes $= \frac{1}{a}$, and $\frac{1}{\varphi} = \frac{m-1}{ma}$, and $\varphi = \frac{ma}{m-1}$, and $\varphi^2\iota$ becomes $\frac{m^2a^2}{(m-1)^2} \times \frac{m-1}{m^3} \times \frac{1}{a^3} \times \frac{e^2}{2}, = \frac{e^2}{2(m-1)ma}$. This is the aberration of extreme parallel rays.

We must now add the refraction of another surface.

Lemma 2. If the focal distance A G be changed by a small quantity G g, the focal distance A H will also be changed by a small quantity H h, and we shall have

$$m \cdot AG^2 : AH^2 = Gg : Hh.$$

Draw M g, M h, and the perpendiculars G i, H k. Then, because the sines of the angles of incidence are in a constant ratio to the sines of the angles of refraction, and the increments of these small angles are proportional to the increments of the sines, these increments of the angles are in the same constant ratio. Therefore,

We have the angle C M g to H M h as m to 1.

Now $\quad Gg : Gi = AG : AM,$

and $\quad Gi : hk = m \cdot AG : HA,$

and $\quad hk : Hh = MA : AH:$

therefore $\quad Gg : Hh = m \cdot AG^2 : AH^2.$

The easiest and most perspicuous method for obtaining the aberration of rays twice refracted, will be to consider the first refraction as not having any aberration, and determine the aberration of the second refraction. Then conceive the focus of the first refraction as shifted by the aberration. This will produce a change in the focal distance of the second refraction, which may be determined by this Lemma.

Prop. II. Let A M, B N (Fig. 10.) be two spherical surfaces, including a refracting substance, and having their centres C and c in the line A G. Let the ray a A pass through the centres, which it will do without refraction. Let another ray m M, tending to G, be refracted by the first surface into M H, cutting the second surface in N, where it is farther refracted into N I. It is required to determine the focal distance BI?

It is plain that the sine of incidence on the second surface is to the sine of refraction into the surrounding air as 1 to m. Also B I may be determined in relation to B H, by means of B H, N x, B c, and $\frac{1}{m}$, in the same way that A H was determined in relation to A G, by means of AG, M X, A C, and m.

Let the radius of the second surface be b, and let e still express the semi-aperture, (because it hardly differs from $N x$.) Also let α be the thickness of the lens. Then observe, that the focal distance of the rays refracted by the first surface, (neglecting the thickness of the lens, and the aberration of the first surface), is the distance of the radiant point for the second refraction, or is the focal distance of rays incident on the second surface. In place of r, therefore, we must take φ; and as we make $k = \frac{1}{a} - \frac{1}{r}$, in order to abbreviate the calculus, let us now make $l = \frac{1}{b} - \frac{1}{\varphi}$; and make $\frac{1}{f} = \frac{1}{b} - m\,l$, as we made $\frac{1}{\varphi} = \frac{1}{a} - \frac{k}{m}$. Lastly, in place of $\iota = \frac{m-1}{m^3}\left(k^3 - \frac{m\,k^2}{r}\right)\frac{e^2}{2}$, make $\iota' = \left(\frac{1}{m} - 1\right)m^3\left(l^3 - \frac{l^2}{m\,\varphi}\right)\frac{e_1^2}{2}, = -\frac{m-1}{m}\left(m^3\,l^3 - \frac{m^2\,l^2}{\varphi}\right)\frac{e^2}{2}$.

Thus we have got an expression similar to the other; and the focal distance B I, after two refractions, becomes B I $\doteq f - f^2\,\iota'$.

But this is on the supposition that B H is equal to φ, whereas it is really $\varphi - \varphi^2\,\iota - \alpha$. This must occasion a change in the value just now obtained of B I. The source of the change is twofold. 1*st*, Because, in the value $\frac{1}{b} - \frac{1}{\varphi}$, we must put $\frac{1}{b} - \frac{1}{\varphi - \varphi^2\,\iota - \alpha}$, and because we must do the same in the fraction $\frac{m^2\,l^2}{\varphi}$. In the second place, when the value of B H is diminished by the quantity $\varphi^2\,\iota + \alpha$, B I will suffer a change in the proportion determined by the 2d Lemma. The first difference may safely be neglected, because the value of ι is very small, by reason of the co-efficient $\frac{e^2}{2}$ being very small, and also because the variation bears a very small ratio to the quantity itself, when the true value of φ differs but little from that of the quantity for which it is employed. The chief change in B I is that which is determined by the Lemma. Therefore take from B I the variation of B H, multiplied by $\frac{m \cdot \text{B I}^2}{\text{B H}^2}$, which is very nearly $= \frac{m f^2}{\varphi^2}$. The product of this multiplication is $m f^2 \iota + \frac{m f^2 \alpha}{\varphi^2}$. This being taken from f, leaves us for the value of B I $f - \frac{f^2 m \alpha}{\varphi^2} - f^2 (m \iota + \nu)$.

In this value, f is the focal distance of an infinitely slender pencil of rays twice refracted by a lens having no thickness, $\alpha \frac{m f^2}{\varphi}$ is the shortening occasioned by the thickness, and $f^2 (m \iota + \nu)$ is the effect of the two aberrations arising from the aperture.

It will be convenient, for several collateral purposes, to exterminate from these formulæ the quantities k, l, and φ. For this purpose make $\frac{1}{n} = \frac{1}{a} - \frac{1}{b}$. We have already $k = \frac{1}{a} - \frac{1}{r}$; and $\frac{1}{\varphi} = \frac{1}{a} - \frac{1}{ma} + \frac{1}{mr}$; and $l = \frac{1}{b} - \frac{1}{\varphi}, = \frac{1}{b} - \frac{1}{a} + \frac{1}{ma} - \frac{1}{mr}$. Now for $\frac{1}{b} - \frac{1}{a}$ write $- \frac{1}{n}$; and we get $l = \frac{1}{ma} - \frac{1}{mr} - \frac{1}{n}$. Therefore $\frac{1}{f} = \frac{1}{b} - ml$ (by construction, page 438, Prop. II.) becomes $= \frac{1}{b} - \frac{1}{a} + \frac{1}{r} + \frac{m}{n}, = \frac{m}{n} + \frac{1}{r}, - \frac{1}{n}, = \frac{m-1}{n} + \frac{1}{r}$.

This last value of $\frac{1}{f}$, (the reciprocal of the focus of a slender pencil twice refracted), viz. $\frac{m-1}{n} + \frac{1}{r}$, is the simplest that can be imagined, and makes n as a substitute for $\frac{1}{a} - \frac{1}{b}$; a most useful symbol, as we shall frequently find in the sequel. It also gives a very simple expression of the focal distance of parallel rays, which we may call the principal focal distance of the lens, and distinguish it in future by the symbol p; for the expression $\frac{1}{f} = \frac{m-1}{n} + \frac{1}{r}$, becomes $\frac{1}{p} = \frac{m-1}{n}$ when the incident light is parallel. And this gives us another very simple and useful measure of f; for $\frac{1}{f}$ becomes $= \frac{1}{p} + \frac{1}{r}$. These equations $\frac{1}{f} = \frac{m-1}{n} + \frac{1}{r}$, $\frac{1}{p} = \frac{m-1}{n}$, and $\frac{1}{f} = \frac{1}{p} + \frac{1}{r}$, deserve therefore to be made very familiar to the mind.

We may also take notice of another property of n. It is half the radius of an isosceles lens, which is equivalent to the lens whose radii are a and b: for suppose the lens to be isosceles, that is, $a = b$; then $n = \frac{1}{a} - \frac{1}{a}$. Now, the second a is negative if the first be positive, or positive if the first be negative. Therefore $\frac{1}{a} - \frac{1}{b} = \frac{a+b}{a^2} = \frac{a+a}{a^2} = \frac{2}{a}$, and $\frac{1}{n} = \frac{2}{a}$, and $n = \frac{a}{2}$. Now the focal distance of this lens is $\frac{m-1}{n}$, and so is that of the other, and they are equivalent.

But, to proceed with our investigation, recollect that we had $\iota = \frac{m-1}{m^3}\left(k^3 - \frac{m k^2}{r}\right)\frac{e^2}{2}$. Therefore $m \iota = \frac{m-1}{m}\left(\frac{k^3}{m} - \frac{k^3}{r}\right)\frac{e^2}{2}$. And ι' was $= \frac{m-1}{m}\left(- m^3 l^3 + \frac{m l^2}{\rho}\right)\frac{e^2}{2}$. Therefore $m \iota' + \iota'$, the aberration (neglecting the thickness of the lens) is $f^2\frac{m-1}{m}\left(\frac{k^3}{m}-\frac{k^2}{r}-m^3 l^3+\frac{m l^2}{\phi}\right)\frac{e^2}{2}$.

If we now write for k, l, and ϕ, their value as determined above, performing all the necessary multiplications, and arrange the terms in such a manner as to collect in one sum the co-efficients of a, n, and r, we shall find 4 terms for the value of $m\iota$, and 10 for the value of ι'. The 4 are destroyed by as many with contrary signs in the value of ι', and there remain 6 terms to express the value of $m\iota+\iota'$, which we shall express by one symbol q; and the equation stands thus:

$$q=\frac{m-1}{m}\left(\frac{m^3}{n^3}-\frac{2m^2+m}{a n^2}+\frac{m+2}{a^2 n}+\frac{3m^2+m}{r n^2}-\frac{4m+4}{a r n}+\frac{3m+2}{r^2 n}\right)\frac{e^2}{2}.$$

The focal distance, therefore, of rays twice refracted, reckoned from the last surface, or B I, corrected for aberration, and for the thickness of the lens, is $f-f^2\frac{m\alpha}{\phi^2}-f^2 q$, consisting of three parts, viz. f, the focal distance of central rays; $f^2\frac{m\alpha}{\phi^2}$, the correction for the thickness of the lens; and $f^2 q$ the aberration.

The above formula appears very complex, but is of very easy management, requiring only the preparation of the simple numbers which form the numerators of the fractions included in the parenthesis. When the incident rays are parallel, the terms vanish which have r in the denominator, so that only the three first terms are used.

We might here point out the cases which reduce the aberration expressed in the formula last referred to, to nothing; but as they can scarcely occur in the object-glass of a telescope, we omit it for the present, and proceed to the combination of two or more lenses.

Lemma 3. If A G be changed by a small quantity Gg, B I suffers a change I i, and Gg : I i = A G^2 : B I^2. For it is well known that the small angles G M g and I N i are equal; and therefore their subtenses G k, I n, are proportional to M G, N I, or to A G, A I, nearly when the aperture is moderate. Therefore we have (nearly)

$$\mathrm{G}k : \mathrm{I}n : \mathrm{AG} : \mathrm{BI}$$
$$\mathrm{I}n : \mathrm{I}i = \mathrm{AM} : \mathrm{BI}$$
$$\mathrm{G}g : \mathrm{G}k = \mathrm{AG} : \mathrm{AM}$$

Therefore $\mathrm{G}g : \mathrm{I}i = \mathrm{AG}^2 : \mathrm{BI}^2$

Prop. III. To determine the focal distance of rays refracted by two lenses placed near to each other on a common axis.

Let A M, B N (Fig. 11.) be the surfaces of the first lense, and C O, D P be the surfaces of the second, and let β be the thickness of the second lens, and δ the interval between them. Let the radius of the anterior surface of the second lens be a', and the radius of its posterior surface be b'. Let m' be to 1 as the sine of incidence to the sine of refraction in the substance of the second lens. Lastly, let p' be the principal focal distance of the second lens. Let the extreme or marginal ray meet the axis in L after passing through both lenses, so that D L is the ultimate focal distance, reckoned from the last surface.

It is plain that D L may be determined by means of a', b', m', p', and C I, in the same manner that B I was determined by means of a, b, m, p, and A G.

The value of B I is $f - m\alpha\frac{f^2}{\varphi^2} - f^2 q$. Take from this the interval δ, and we have C I $= f - m\alpha\frac{f^2}{\varphi^2} - \delta - f^2 q$. Let the small part $- m\alpha\frac{f^2}{\varphi^2} - \delta - f^2 q$ be neglected for the present, and let C I be supposed $= f$. As we formed

φ, f, and q, by means of a, b, m, n, and r, let us now form $\varphi' f'$, and q', for the second lens, by means of a', b', m', n', $\left(=\frac{1}{a'}-\frac{1}{b'}\right)$, and r'. φ' will be the focal distance of a slender pencil refracted by the first surface, f will be the focal distance of this pencil after two refractions, and q' will be the co-efficient of the aberration, neglecting the thickness and interval of the lenses.

Proceeding in this way, D L will be $=f'-m\beta\frac{f'^2}{\varphi'}-f'^2q$. But because CI is really less than f, by the quantity $m\alpha\frac{f^2}{\varphi'^2}+\delta+f^2q'$, we must (by Lemma 3.) subtract the product of this quantity, multiplied by $\frac{\mathrm{DL}}{\mathrm{BI}^2}$, (which is nearly $\frac{f'^2}{f^2}$), from $f'-m\beta\frac{f'^2}{\varphi^2}-f'^2q'$.

By this process, we shall have

$$\mathrm{DL}=f'-f'^2\left(\frac{m\alpha}{\varphi^2}+\frac{\delta}{f^2}+\frac{m'\beta}{\varphi'^2}\right)-f'^2(q+q').$$

The first term f' of this value of D I is the focal distance of a slender pencil of central rays refracted by both lenses, neglecting their thickness and distance; the second term, $-f'^2\left(\frac{m\alpha}{\varphi^2}+\frac{\delta}{f^2}+\frac{m'\beta}{\varphi'^2}\right)$ is the correction necessary for these circumstances; and the third term, $-f'^2(q+q')$, is the correction for the aperture $2e$. And it is evident that q' is a formula precisely similar to q, containing the same number of terms, and differing only by the m', a', n', and r', employed in place of m, a, n, and r.

It is also evident, that if there be a third lens, we shall obtain its focal distance by a process precisely similar to that by which we obtained D L; and so on for any number of lenses.

Thus have we obtained formulæ by which the foci of rays are determined in the most general terms; and in such a manner as shall point out the connexion of the curvatures, thicknesses, and distances of the lenses, with their spherical aberrations, and with the final aberration of the compound lens, and give the aberrations in separate symbols, so that we can treat them by themselves, and subject them to any conditions which may enable us to correct one of them by another.

We also see, in general, that the correction for the thickness and distance of the lenses are exhibited in terms which involve only the focal distances of central rays, and have very little influence on the aberrations, and still less on the ratio of the aberrations of the different lenses. This is a most convenient circumstance; for we may neglect them while we are determining q and q', and in determining the ratio of the focal distances of the several lenses, on which the correction of the chromatic aberration chiefly depends. Therefore, in the construction of a compound lens for uniting the different colours, we may neglect this correction for the thickness and distance till the end of the process. When we apply it, we shall find that it chiefly affects the final focal distance, making it somewhat longer, but has hardly any influence either on the chromatic or spherical aberration. We do not hesitate to say, that the final formulæ here given are abundantly accurate, while they are vastly more manageable than those employed by Euler or D'Alembert. We have calculated trigonometrically the progress of the rays through one of the glasses, which will be given as an example, giving it a very extravagant aperture, that the errors of the formulæ might be very remarkable. We found the real aberration exceed the aberration assigned by the formula by no more than $\frac{1}{79}$ part, a difference which is quite insignificant. The process here given derives its simplicity from the frequent occurrence of harmonic proportions in all optical theorems. This enabled Mr Clairaut to employ the reciprocals of the radii and distances with so much simplicity and generality.

We consider it as another advantage of Mr Clairaut's method, that it gives, by the way, formulæ for the more ordinary questions in optics, which are of wonderful simplicity, and most easily remembered. The chief problems in the elementary construction of optical instruments relate to the focal distances of central rays. This determines the focal distances and arrangement of the glasses. All the rest may be called the refinement of optics; teaching us how to avoid or correct the indistinctness, the colours, and the distortions, which are produced in the images formed by these simple constructions. We shall mention a few of these formulæ which occur in our process, and tend greatly to abbreviate it when managed by an experienced analyt.

Let m be to 1 as the sine of incidence to the sine of refraction; let a and b be the radii of the anterior and posterior surfaces of a lens; let r be the distance of the radiant point, or the focus of incident central rays, and f the distance of the conjugate focus; and let p be the principal focal distance of the lens, or the focal distance of parallel rays. Make $\frac{1}{n}$ equal to $\frac{1}{a}-\frac{1}{b}$; let the same letters, a', b', r', &c. express the same things for a second lens; and a'', b'', r'', &c. express them for a third; and so on. Then we have

$$\frac{1}{f}=\frac{m-1}{n}+\frac{1}{r};\ \frac{1}{f'}=\frac{m'-1}{n'}+\frac{1}{r'};\ \frac{1}{f''}=\frac{m''-1}{n}+\frac{1}{r''},\ \&c.$$

Therefore when the incident light is parallel, and r infinite, we have $\frac{1}{p}=\frac{m-1}{n}$; $\frac{1}{p'}=\frac{m'-1}{n'}$; $\frac{1}{p''}=\frac{m''-1}{n''}$, &c.

And when several lenses are contiguous, so that their intervals may be neglected, and therefore $\frac{1}{f}$, belonging to the first lens, becomes $\frac{1}{r'}$, belonging to the second, we have

1. $\frac{1}{r'} = \frac{1}{f'}, = \frac{m-1}{n} + \frac{1}{r}, = \frac{1}{p} + \frac{1}{r}.$

2. $\frac{1}{r''}, = \frac{1}{f''}, = \frac{m'-1}{n'} + \frac{m-1}{n} + \frac{1}{r}, = \frac{1}{p'} + \frac{1}{p} + \frac{1}{r}.$

3. $\frac{1}{f'''} = \frac{m''-1}{n''} + \frac{m'-1}{n} + \frac{m-1}{n} + \frac{1}{r}, = \frac{1}{p''} + \frac{1}{p'} + \frac{1}{p} + \frac{1}{r}.$

Nothing can be more easily remembered than these formulæ, how numerous soever the glasses may be.

Having thus obtained the necessary analysis and formula, it now remains to apply them to the construction of achromatic lenses; in which it fortunately happens, that the employment of several surfaces, in order to produce the union of the differently refrangible rays, enables us at the same time to employ them for correcting each other's spherical aberration.

A white or compounded ray is separated by refraction into its component coloured rays, and they are diffused over a small angular space. Thus it appears, that the glass used by Sir Isaac Newton in his experiments diffused a white ray, which was incident on its posterior surface in an angle of 30°, in such a manner that the extreme red ray emerged into air, making an angle of 50° 21⅓′ with the perpendicular; the extreme violet ray emerged in an angle of 51° 15⅗′; and the ray which was in the confines of green and blue, emerged in an angle of 50° 48⅔′ If the sine of the angle 30° of incidence be called 0,5, which it really is, the sine of the emergence of the red ray will be 0,77; that of the violet ray will be 0,78; and that of the intermediate ray will be 0,77½, an exact mean between the two extremes. This ray may therefore be called the mean refrangible ray, and the ratio of 77½ to 50, or of 1,55 to 1, will very properly express the mean refraction of this glass; and we have for this glass $m = 1{,}55$. The sine of refraction, being measured on a scale, of which the sine of incidence occupies 100 parts, will be 154 for the red ray, 155 for the mean ray, and 156 for the violet ray. This number, or its ratio to unity, is commonly taken to represent the refractive power of the glass. There is some impropriety in this, unless we consider ratios as measured by their logarithms; for if m be 1, the substance does not refract at all. The refractive power can be properly measured only by the refraction which it produces; that is, by the change which it makes in the direction of the light, or the angle contained between the incident and refracted rays. If two substances produce such deviations always in one proportion, we should then say that their refractive powers are in that proportion. This is not true in any substances; but the sines of the angles, contained between the refracted ray and the perpendicular, are always in one proportion when the angle of incidence in both substances is the same. This being a cognisable function of the real refraction, has therefore been assumed as the only convenient measure of the refractive powers. Although it is not strictly just, it answers extremely well in the most usual cases in optical instruments: the refractions are moderate; and the sines are very nearly as the angles contained between the rays and the perpendicular; and the real angles of refraction, or deflections of the rays, are almost exactly proportional to $m-1$. The most natural and obvious measure of the refractive powers would therefore be $m-1$. But this would embarrass some very frequent calculations; and we therefore find it best, on the whole, to take m itself for the measure of the refractive power.

The separation of the red, violet, and intervening rays, has been called *dispersion*; and although this arises merely from a difference of the refractive power in respect of the different rays, it is convenient to distinguish this particular modification of the refractive power by a name, and we call it the DISPERSIVE POWER of the refracting substance.

It is susceptible of degrees; for a piece of flint glass will refract the light, so that when the sine of refraction of the red ray is 77, the sine of the refraction of the violet ray is nearly 78½; or if the sine of refraction of the red ray, measured on a particular scale, is 1,54, the sine of refraction of the violet ray is 1,57. The dispersion of this substance, being measured by the difference of the extreme sines of refraction, is greater than the dispersion of the other glass, in the proportion of 3 to 2.

But this alone is not a sufficient measure of the absolute dispersive power of a substance. Although the ratio of 1,54 to 1,56 remains constant, whatever the real magnitude of the refractions of common glass may be, and though we therefore say that its dispersive power is constant, we know, that by increasing the incidence and the refraction, the absolute dispersion is also increased. Another substance shows the same properties, and in a particular case may produce the same dispersion; yet it has not for this sole reason the same dispersive power. If indeed the incidence and the refraction of the mean ray be also the same, the dispersive power cannot be said to differ; but if the incidence and the refraction of the mean ray be less, the dispersive power must be considered as greater, though the actual dispersion be the same; because if we increase the incidence till it becomes equal to that in the common glass, the dispersion will now be increased. The proper way of conceiving the dispersion therefore is, to consider it as a portion of the whole refraction; and if we find a substance making the same dispersion with half the general refraction, we must say that the dispersive quality is double; because by making the refraction equal, the dispersion will really be double.

If therefore we take m as a symbol of the separation of the extreme rays from the middle ray, $\frac{m}{m-1}$ is the natural measure of the dispersive power. We shall express this in the Leibnitzian notation, thus $\frac{dm}{m-1}$, that we may avoid the indistinctness which the Newtonian notation would occasion when m is changed for m' or m''.

It is not unusual for optical writers to take the whole separation of the red and violet rays for the measure of the dispersive power, and to compare this with the refracting power with respect to one of the extreme rays. But it is surely better to consider the mean refraction as the measure of the refracting power; and the deviation of either of the extremes from this mean is a proper enough measure of the dispersion, being always half of it. It is attended with this convenience, that being introduced into our computations as a quantity infinitely small, and treated as such for the ease of computation, while it is really a quantity of sensible magnitude; the errors arising from this supposition are diminished greatly, by taking one half of the deviation and comparing it with the mean refraction. This method has, however, this inconvenience, that it does not exhibit at once the refractive power in all substances respecting any particular colour of light, for it is not the ray of any particular colour that suffers the mean refraction. In common glass it is the ray which is in the confines of the yellow and blue; in flint-glass it is nearly the middle blue ray; and in other substances it is a different ray. These circumstances appear plainly in the different proportions of the colours of the prismatic spectrum exhibited by different substances. This will be considered afterwards, being a great bar to the perfection of achromatic instruments.

The way in which an achromatic lens is constructed, is to make use of a contrary refraction of a second lens to destroy the dispersion or spherical aberration of the first.

The first purpose will be answered if $\frac{dm}{n}$ be equal to $-\frac{dm'}{n'}$. For, in order that the different coloured rays may be collected into one point by two lenses, it is only necessary that $\frac{1}{f'}$, the reciprocal of the focal distance of rays refracted by both, may be the same for the extreme and mean rays, that is, that $\frac{m + dm - 1}{n} + \frac{m' + dm' - 1}{n'} + \frac{1}{r}$; be of the same value with $\frac{m - 1}{n} + \frac{m' - 1}{n'} + \frac{1}{r}$; which must happen if $\frac{dm}{n} + \frac{dm'}{n'}$ be $= 0$, or $\frac{dm}{n} = - \frac{dm'}{n}$. This may be seen in another way, more comprehensible by such as are not versant in these discussions. In order that the extreme colours which are separated by the first lens may be rendered parallel by the second, we have shown already that n and n' are proportional to the radii of the equivalent isosceles lenses, being the halves of these radii. They are therefore (in these small refractions) inversely proportional to the angles formed by the surfaces at the edges of the lenses. n' may therefore be taken for the angle of the first lens, and n for that of the second. Now the small refraction by a prism, whose angle (also small) is n', is $\overline{m - 1} \times n'$. The dispersive power being now substituted for the refractive power, we have for this refraction of the prism $dm \times n'$. This must be destroyed by the opposite refraction of the other prism $dm' \times n$. Therefore $dm \times n' = dm' \times n$, or $\frac{dm}{n} = - \frac{dm'}{n'}$. In like manner, this effect will be produced by three lenses if $\frac{dm}{n} + \frac{dm'}{n'} + \frac{dm''}{n''}$ be $= 0$, &c.

Lastly, the errors arising from the spherical figure which we expressed by $- R^2 (q + q)$ will be corrected if $q + q'$ be $= 0$. We are therefore to discover the adjustments of the quantities employed in the preceding formulæ, which will insure these conditions. It will render the process more perspicuous if we collect into one view the significations of our various symbols, and the principal equations which we are to employ.

1. The ratios to unity of the sines of mean incidence in the different media are m, m', m''

2. The ratio of the differences of the sines of the extremes - - - $\frac{dm}{dm'}, = u$.

3. The ratio $\frac{m-1}{m'-1}$ - - - $= c$

4. The radii of the surfaces $a, b; a', b; a'', b''$.

5. The principal focal distances, or the focal distances of parallel central rays, p, p', p''.

6. The focal distance of the compound lens P.

7. The distance of the radiant point, or of the focus of incident rays on each lens r, r', r''.

8. The focal distance of the rays refracted by each lens - - - f, f', f''.

9. The focal distance of rays refracted by the compound lens - - - F.

10. The half breadth of the lens e.

Also the following subsidiary values:

1. $\frac{1}{n} = \frac{1}{a} - \frac{1}{b};\ \frac{1}{n'} = \frac{1}{a'} - \frac{1}{b'};\ \frac{1}{n''} = \frac{1}{a''} - \frac{1}{b''}$.

2. $q = \frac{m-1}{m}\left(\frac{m'^3}{n^3} - \frac{2m^2+m}{an^2} + \frac{m+2}{a^2n} + \frac{3m^2+m}{rn^2} - \frac{4(m+1)}{arn} + \frac{3m+2}{r^2n}\right)\frac{e^2}{2}$. And q' and q'' must be formed in the same manner from m', a', n', r'; and from m'', a'', n'', r'', as q is formed from m, a, n, r.

3. Also, because in the case of an object-glass, r is infinitely great, the last term $\frac{1}{r}$ in all the values of $\frac{1}{f}, \frac{1}{f'}, \frac{1}{f''}$, $\frac{1}{r'}, \frac{1}{r''}$, will vanish, and we shall also have F = P.

Therefore, in a double object-glass, $\frac{1}{\mathrm{P}}=\frac{m'-1}{n'}+\frac{m-1}{n}$, $=\frac{1}{p}+\frac{1}{p'}$.

And in a triple object-glass $\frac{1}{\mathrm{P}}=\frac{m''-1}{n''}+\frac{m'-1}{n}+\frac{m-1}{a}$, $=\frac{1}{p''}+\frac{1}{p'}+\frac{1}{p}$.

Also, in a double object-glass, the correction of spherical aberration requires $q+q'=v$.

And a triple object-glass requires $q+q'+q''=v$. For the whole error is multiplied by F^2, and by $\frac{1}{2}e^2$; and therefore the equation which corrects this error may be divided by $\mathrm{F}^2\,\frac{1}{2}\,e^2$.

This equation, in the preceding page, 7th line from the bottom, giving the value of q, q', q'', may be much simplified as follows: In the first place, they may be divided by m, m', or m'', by applying them properly to the terms within the parenthesis, and expunging them from the denominator of the general factors $\frac{m-1}{m}$, $\frac{m'-1}{m'}$, $\frac{m''-1}{m''}$. This does not alter the values of q, q', and q''. In the second place, the whole equations may be afterwards divided by $m'-1$. This will give the values of $\frac{q}{m'-1}$, $\frac{q'}{m'-1}$, and $\frac{q''}{m'-1}$, which will still be equal to nothing if $q+q'+q''$ be equal to nothing.

This division reduces the general factor $\frac{m'-1}{m'}$ of q' to $\frac{1}{m'}$. And in the equation for q we obtain, in place of the general factor $\frac{m-1}{m}$, the factor $\frac{m-1}{m'-1}$, or c. This will also be the factor of the value of q'' when the third lens is of the same substance with the first, as is generally the case.

And, in the third place, since the rays incident on the first lens are parallel, all the terms vanish from the value of q, in which $\frac{1}{r}$ is found, and there remain only the three first, viz. $\frac{m^2}{n^3} - \frac{2m^2+m}{a n^2} + \frac{m+2}{a^2 n}$.

Performing these operations, we have

$$\frac{q}{m'-1} = c\left(\frac{m^2}{n^3} - \frac{2m+1}{a n^2} + \frac{m+2}{m a^2 n}\right)\frac{e^2}{2}$$

$$\frac{q'}{m'-1} = \left(\frac{m'^2}{n'^3} - \frac{2m'+1}{a' n'^2} + \frac{m'+2}{m' a'^2 n'} + \frac{3m'+1}{r' n'^2} - \frac{4(m'+1)}{m' a' r' n'} + \frac{3m'+2}{m' r'^2 n'}\right)\frac{e^2}{2}$$

$$\frac{q''}{m'-1} = c\left(\frac{m^2}{n''^3} - \frac{2m+1}{a'' n''^2} + \frac{m+2}{m'' a''^2 n''} + \frac{3m+1}{r'' n''^2} + \frac{4(m+1)}{m'' a'' r'' n''} + \frac{3m+2}{m'' r''^2 n}\right)\frac{e^2}{2}$$

Let us now apply this investigation to the construction of an object-glass; and we shall begin with a double lens.

Construction of a Double Achromatic Object-Glass.

Here we have to determine four radii a, b, a', and b'. Make $n = 1$. This greatly simplifies the calculus, by exterminating it from all the denominators. This gives for the equation $\frac{dm}{n} + \frac{dm'}{n'} = 0$, the equation $dm + \frac{dm'}{n'} = 0$, or $dm = -\frac{dm'}{n'}$, and $\frac{1}{n'} = -\frac{dm}{dm'}$, $= -u$. Also we have r, the focal distance of the light incident on the second lens, the same with the principal focal distance p of the first lens (neglecting the interval, if any). Now $\frac{1}{p} = \frac{m-1}{n}$, which, in the present case, is $= m-1$. Also $\frac{1}{p'}$ is $= -u\,(m'-1)$, and $\frac{1}{P} = m-1-u\,(m'-1) = w$.

Make these substitutions in the values of $\frac{q}{m-1}$ and $\frac{q'}{m'-1}$, and we obtain the following equation:

$$cm^2 - \frac{c\,(2\,m+1)}{a} + \frac{c\,(m+2)}{m\,a^2} - u^{3\prime}\,m^2 - \frac{u^2\,(2m'+1)}{a'} - \frac{u\,(m'+2)}{m'\,a'^2} + u^2\,(3\,m'+1)\,(m-1) + \frac{4u\,(m'+1)\,(m-1)}{m'\,a'} - \frac{u\,(3\,m'+2)\,(m-1)^2}{m'} = 0.$$

Arrange these terms in order, according as they are factors of $\frac{1}{a^2}$, $\frac{1}{a}$, $\frac{1}{a'^2}$, $\frac{1}{a'}$, or independent quantities. It puts on this form:

$$\frac{c\,(m+2)}{m} \times \frac{1}{a^2} - c\,(2\,m+1) \times \frac{1}{a} - \frac{u\,(m'+2)}{m'} \times \frac{1}{a'^2} - \left(u^2\,(2\,m'+1) - \frac{4u\,(m'+1)\,(m-1)}{m'}\right) \times \frac{1}{a'} + c\,m^2 + u^2\,(3m'+1)\,(m-1) - u^3\,m'^2 - \frac{u\,(3m'+2)\,(m-1)^2}{m'} = 0.$$

Let A be the coefficient of $\frac{1}{a^2}$, B that of $\frac{1}{a}$, C that of $\frac{1}{a'^2}$, D that of $\frac{1}{a'}$, and E the sum of the independent quantity; that is, let A be $= \frac{c\,(m+2)}{m}$, B $= c\,(2\,m+1)$, C $= \frac{u\,(m'+2)}{m'}$, D $= u^2\,(2\,m'+2) - \frac{4\,u\,(m'+1)\,(m-1)}{m'}$, and E $= c\,m^2 + u^2\,(3\,m'+1)\,(m-1) - u^3\,m'^2 - \frac{u\,(3\,m'+2)\,(m-1)^2}{m'}$.

Our final equation becomes

$$\frac{A}{a^2} - \frac{B}{a} - \frac{C}{a'^2} - \frac{D}{a'} + E = 0.$$

The coefficients of this equation, and the independent quantity, are all known, from our knowledge of m, m', dm,

dm; and we are to find the values of a and a', and from them and $n = 1$ to find the values of b and b'.

But it is evidently an indeterminate equation, because there are two unknown quantities; so that there may be an infinity of solutions. It must be rendered determinate by means of some other conditions to which it may be subjected. These conditions must depend on some other circumstances which may direct our choice.

One circumstance occurs to us which we think of very great consequence. In the passage of light from one substance to another, there is always a considerable portion reflected from the posterior surface of the first, and from the anterior surface of the last; and this reflection is more copious in proportion to the refraction. This loss of light will therefore be diminished by making the internal surfaces of the lenses to coincide; that is, by making $b = a'$. This will be attended with another advantage. If we put between the glasses a substance of nearly the same refracting power, we shall not only completely prevent this loss of light, but we shall greatly diminish the errors which arise from an imperfect polish of the surfaces. We have tried this, and find the effect very surprising. The lens being polished immediately after the figure has been given it, and while it was almost impervious to light by reason of its roughness, which was still sensible to the naked eye, performed as well as when finished in the finest manner.

N. B. This condition, by taking away one refraction, obliges us to increase those which remain, and therefore increases the spherical aberrations. And since our formula does not fully remove those (by reason of the small quantities neglected in the process), it is uncertain whether this condition be the most eligible. We have, however, no direct argument to the contrary.

Let us see what determination this gives us.

In this case $\frac{1}{a'} = \frac{1}{b} = \frac{1}{a} - 1$. For because $\frac{1}{n} = \frac{1}{a} -$

$\frac{1}{b}$ and $n = 1$, we have $1 + \frac{1}{b'}, = \frac{1}{a'}$, and $\frac{1}{b} = \frac{1}{a} - 1$. Therefore $\frac{1}{a'^2} = \frac{1}{a^2} - \frac{2}{a} + 1$. Therefore, in our final equation, put $\frac{1}{a^2} - \frac{2}{a} + 1$ in place of $\frac{1}{a'^2}$ and $\frac{1}{a} - 1$ in place of $\frac{1}{a'}$, and it becomes $\frac{A - C}{a^2} - \frac{B + D - 2\,C}{a} + E + D - C = 0$.

Thus have we arrived at a common affected quadratic equation, where $\frac{1}{a}$ is the unknown quantity. It has the common form $p\,x^2 + q\,x + r = 0$, where p is $= A - C$, q is equal to $2\,C - B - D$, r is equal to $E + D - C$, and x is equal to $\frac{1}{a}$.

Divide the equation by p, and we have $x^2 + \frac{q}{p}\,x + \frac{r}{p} = 0$. Make $s = \frac{q}{p}$ and $t = \frac{r}{p}$, and we have $x^2 + s\,x + t = 0$. This gives us finally $\frac{1}{a}$, or $x = -\frac{1}{2}\,s \pm \sqrt{\frac{1}{4}\,s^2 - t}$.

This value of $\frac{1}{a}$ is taken from a scale of which the unit is half the radius of the isosceles lens, which is equivalent to the first lens, or has the same focal distance with it. We must then find (on the same scale) the value of b, *viz.* $\frac{1}{a} - t$, which is also the value of a'. Having obtained a'', we must find b' by means of the equation $\frac{1}{n'} = \frac{1}{a'} - \frac{1}{b'}$ and therefore $\frac{1}{b} = \frac{1}{a'} - \frac{1}{n'}$. But $\frac{1}{n'} = u$. Therefore $\frac{1}{b'} = \frac{1}{a'} + u, = \frac{1}{a} + u - 1$.

Thus is our object-glass constructed; and we must determine its focal distance, or its reciprocal $\frac{1}{P}$. This is $= m - 1 - u\,(m' - 1.)$

All these radii and distances are measured on a scale of which n is the unit. But it is more convenient to measure every thing by the focal distance of the compound object-glass. This gives us the proportion which all the distances bear to it. Therefore calling P unity, in order to obtain $\frac{1}{a}$ on this scale, we have only to state the analogy $m - 1 - u\,(m' - 1) : 1 = \frac{1}{a} : \frac{1}{A}$, and A is the radius of our first surface measured on a scale of which P is the unit.

If, in the formula which expresses the final equation for $\frac{1}{a}$, the value of t should be positive, and greater than $\frac{1}{4}s^2$, the equation has imaginary roots; and it is not possible with the glasses employed, and the conditions assumed, to correct both the chromatic and spherical aberrations.

If t is negative and equal to $\frac{1}{4}s^2$, the radical part of the value is $= 0$, and $\frac{1}{a} = -\frac{1}{2}s$. But if it be negative or positive, but less than $\frac{1}{4}s^2$, the equation has two real roots, which will give two constructions. That is to be preferred which gives the smallest curvature of the surfaces; because, since in our formulæ which determine the spherical aberration some quantities are neglected, these quantities are always greater when a large arch (that is, an arch of many degrees) is employed. No radius should be admitted which is much less than $\frac{1}{3}$ of the focal distance.

All this process will be made plain and easy by an example.

Very careful experiments have shown, that in common crown-glass the sine of incidence is to the sine of refraction as 1,526 is to 1, and that in the generality of flint-glass it is as 1,604 to 1. Also that $\frac{dm}{dm'} = 0,6054 = u$. Therefore $m - 1 = 0,526$; $m' - 1 = 0,604$; $c = \frac{m - 1}{m' - 1'} = 0,87086$. By these numbers we can compute the coefficients of our final equation. We shall find them as follows:—

$A = 2,012$
$B = 3,529$
$C = 1,360$
$D = -0,526$
$E = 1,8659$

The general equation (p. 455. l. 19.), when subjected to the assumed coincidence of the internal surfaces, is $\frac{A - C}{a^2} - \frac{B + D - 2C}{a} + E + D - C = 0$. $A - C$ is $= 0,652$; $B + D - 2C$ is $= 0,283$; and $E + D - C$ is $= -0,020$; and the equation with numerical coefficients is $\frac{0,652}{a^2} - \frac{0,283}{a} - 0,020 = 0$, which corresponds to the equation $p x^2 + q x + r = 0$. We must now make $s = \frac{q}{p}, = \frac{0,283}{0,652}, = 0,434$, and $t = \frac{r}{p}, = \frac{0,02}{0,652}, = 0,0307$. This gives us the final quadratic equation $\frac{1}{a^2} - \frac{0,434}{a} - 0,0307 = 0$. To solve this, we have $-\frac{1}{2} s = 0,217$, and $\frac{1}{4} s^2 - 0,0471$. From this take t, which is $= -0,0307$ (that is, to 0,0471 add 0,0307), and we obtain 0,0778, the square root of which is $= 0,2789$. Therefore, finally, $\frac{1}{a} = 0,2170 \pm 0,2789$, which is either 0,4959 or $-0,0619$. It is plain that the first must be preferred, because the second gives a negative radius, or makes the first surface of the crown-glass concave. Now, as the convergence of the rays is to be produced by the crown-glass, the other surface must become very convex, and occasion great errors in the computed aberration. We therefore retain 0,4959 for the value of $\frac{1}{a}$, and a is $= \frac{1}{0,4959}$, $= 2,0166$.

To obtain b, use the equation $\frac{1}{b} = \frac{1}{a} - 1$, which gives $\frac{1}{b} = -0,5041$, and therefore a convex surface. b is therefore $= \frac{1}{0,5041}$, $= 1,9837$.

a' is the same with b, and $\frac{1}{a'} = -0,5041$.

To obtain b', use the equation $\frac{1}{b'} = \frac{1}{a'} + u$. Now $u = 0,6054$, and $\frac{1}{a'} = -0,5041$. The sum of these is 0,1013; and since it is positive, the surface is concave. $b' = \frac{1}{,1013} = 9,872$.

Lastly, $\frac{1}{P} = m - 1 - u\,(m' - 1) = 0,1603$, and $P = \frac{1}{0,1603}$, $= 0,2383$.

Now, to obtain all the measures in terms of the focal distance P, we have only to divide the measures already found by 6,2383, and the quotients are the measures wanted.

$$\text{Therefore } a = \frac{2,0166}{6,2383} = 0,32325$$

$$b = \frac{1,9837}{6,2383} = -0,31798$$

$$a' = \;\text{- -}\; = -0,31798$$

$$b' = \frac{9,872}{6,2383} = \;\; 1,5825$$

$$P = \;\text{- - - -}\;\; 1.$$

If it be intended that the focal distance of the object-glass shall be any number n of inches or feet, we have only to multiply each of the above radii by n, and we have their lengths in inches or feet.

Thus we have completed the investigation of the construction of a double object-glass. Although this was intricate, the final result is abundantly simple for practice, especially with the assistance of logarithms. The only troublesome thing is the preparation of the numerical coefficients A, B, C, D, E of the final equation. Strict attention must also be paid to the positive and negative signs of the quantities employed.

We might propose other conditions. Thus it is natural to prefer for the first or crown-glass lens such a form as shall give it the smallest possible aberration. This will require a small aberration of the flint-glass to correct it. But a little reflection will convince us that this form will not be good. The focal distance of the crown-glass must not exceed one-third of that of the compound glass; these two being nearly in the proportion of $d\,m' - d\,m$ to $d\,m'$. Therefore if this form be adopted, and a be made about $\frac{1}{6}$th of b, it will not exceed $\frac{1}{5}$th of P. Therefore, although we may produce a most accurate union of the central and marginal rays by opposite aberrations, there will be a considerable aberration of some rays which are between the centre and the margin.

It is absolutely impossible to collect into one point the whole rays (though the very remotest rays are united with the central rays), except in a very particular case, which cannot obtain in an object-glass; and the small quantities which are neglected in the formula which we have given for the spherical aberration, produce errors which do not follow any proportion of the aperture which can be expressed by an equation of a manageable form. When the aperture is very large, it is better not to correct the aberration for the whole aperture, but for about $\frac{5}{6}$ths of it. When the rays corresponding to this distance are made to coincide with the central rays by means of apposite aberrations, the rays which are beyond this distance will be united with some of those which are nearer to the centre, and the whole diffusion will be considerably diminished. Dr Smith has illustrated this in a very perspicuous manner in his theory of Catoptric Microscope.

But although we cannot adopt this form of an object-glass, there may be other considerations which may lead us to prefer some particular form of the crown-glass, or of the flint-glass. We shall therefore adapt our general equation $\frac{A}{a^2} - \frac{B}{a} - \frac{C}{a^2} - \frac{D}{a'} + E = 0$ to this condition.

Therefore let h express this selected ratio of the two radii of the crown-glass, making $\frac{a}{b} = h$ (remembering always that a is positive and b negative in the case of a double convex, and h is a negative number.)

With this condition we have $\frac{1}{b} = \frac{h}{a}$. But when we make n the unit of our formula of aberration, $\frac{1}{b} = \frac{1}{a} - 1$. Therefore $1 = \frac{1}{a} - \frac{h}{a}$, and $\frac{1}{a} = \frac{1}{1-h}$. Now substitute this for $\frac{1}{a}$ in the general equation, and change all the signs (which still preserves it $= 0$.), and we obtain

$$\frac{C}{a'^2} + \frac{D}{a'} - E - \frac{A}{(1-h)^2} + \frac{B}{1-h} = 0.$$

By this equation we are to find $\frac{1}{a}$, or the radius of the anterior surface of the flint-glass. The equation is of this form $p\,x^2 + q\,x + r = 0$, and we must again make $s = \frac{q}{p}$, and $t = \frac{r}{p}$. Therefore $s = \frac{D}{C}$, and $t = \frac{1}{C} \times \left(\frac{B}{1-h} - \frac{A}{(1-h)^2} - E\right)$. Then, finally,

$$\frac{1}{a'} = -\tfrac{1}{2}s \pm \sqrt{\tfrac{1}{4}s^2 - t}.$$

It may be worth while to take a particular case of this condition. Suppose the crown-glass to be of equal convexities on both sides. This has some advantages: We can tell with precision whether the curvatures are precisely equal, by measuring the focal distance of rays reflected back from its posterior surface. These distances will be precisely equal. Now it is of the utmost importance in the construction of an object-glass, which is to correct the spherical aberration, that the forms be precisely such as are required by our formulæ.

In this case of a lens equally convex on both sides $\frac{1}{a}$ is $= -\frac{1}{b}$, $= \frac{1}{2}$. Substitute this value for $\frac{1}{a}$ in the general equation $\frac{A}{a^2} - \frac{B}{a} - \frac{C}{a'^2} - \frac{D}{a'} + E = 0$, and then $\frac{A}{a^2} = \frac{A}{4}$; $\frac{B}{a}$ becomes $\frac{B}{2}$. Now change all the signs, and we have $\frac{C}{a'2} + \frac{D}{a'} - E - \frac{A}{4} + \frac{B}{2} = 0$, by which we are to find a'. This in numbers is $\frac{1{,}360}{a'} - \frac{0{,}526}{a'} - 0{,}6044 = 0$. Then $s = \frac{-0{,}526}{1{,}360}, = 0{,}3867$, and $t = \frac{-0{,}6044}{1{,}360}, = -0{,}4444$. Then $-\frac{1}{2}s = 0{,}1933$; $\frac{1}{4}s^2 = 0{,}0374$; and $\sqrt{\frac{1}{4}S^2 - t} = \pm 0{,}6941$; so that $\frac{1}{a'} = 0{,}1933 \pm 0{,}6941$. This gives two real roots, viz. 0,8874, and — 0,5008. If we take the first, we shall have a convex anterior surface for the flint-glass, and consequently a very deep concave for the posterior surface. We therefore take the second or negative root — 0,5008.

We find $\frac{1}{b'}$, as before, by the equation $\frac{1}{b'} = \frac{1}{a'} + u$, $= 0,1046$, which will give a large value of b'.

We had $\frac{1}{a} = \frac{1}{2}$

and $\frac{1}{b} = -\frac{1}{2}$

and $\frac{1}{P}$ is the same as in the former case, viz. 0,1603.

Having all these reciprocals, we may find a, b, a' b', and P; and then dividing them by P, we obtain finally

$$\begin{aligned} a &= 0,3206 \\ b &= -0,3206 \\ a' &= -0,3201 \\ b' &= 1,533 \\ P &= 1. \end{aligned}$$

By comparing this object-glass with the former, we may remark, that diminishing a a little increases b, and in this respect improves the lens. It indeed has diminished b', but this being already considerable, no inconvenience attends this diminution. But we learn, at the same time, that the advantage *must* be very small; for we cannot diminish a much more, without making it as small as the smallest radius of the object-glass. This proportion is therefore very near the maximum, or best possible; and we know that in such cases, even considerable changes in the radii will make but small changes in the result: for these reasons we are disposed to give a strong preference to the first construction, on account of the other advantages which we showed to attend it.

As another example, we may take a case which is very nearly the general practice of the London artists. The radius of curvature for the anterior surface of the convex crown-glass is $\frac{5}{6}$ths of the radius of the posterior surface, so that $h = \frac{5}{6}$. This being introduced into the determinate equation, gives

$$a = 0{,}2938 \qquad a' = -0{,}3443$$
$$b = -0{,}3526 \qquad b = 1{,}1474$$

As another condition, we may suppose that the second or flint-glass is of a determined form.

This case is solved much in the same manner as the former. Taking h to represent the ratio of a' and b', we have $\frac{1}{a'} = \frac{1}{1-h}$. This value being substituted in the general equation $\frac{A}{a^2} - \frac{B}{a} - \frac{C}{a'^2} - \frac{D}{a} + E = 0$, gives us $\frac{A}{a^2} - \frac{B}{a} + E - \frac{C}{(1-h)^2} - \frac{D}{1-h} = 0$. This gives for the final equation $x^2 + sx + t = 0$, $s = \frac{B}{A}$, and $t = \frac{1}{A} \times \left(E - \frac{C}{(1-h)^2} - \frac{D}{1-h}\right)$ and $\frac{1}{a} = -\frac{1}{2}s \pm \sqrt{\frac{1}{4}s^2 - t}$.

We might here take the particular case of the flint-glass being equally concave on both sides. Then, because $\frac{1}{n'} = -u$, and in the case of equal concavities $\frac{2}{a'} = \frac{1}{n'}, = -u$, it is sufficient to put $-\frac{1}{2}u$ for $\frac{1}{a'}$. This being done, the equation becomes $\frac{A}{a} - \frac{B}{a}\frac{Cu^2}{4} + \frac{Du}{2} + E = 0$. This gives $s = \frac{B}{A}$, and $t = \frac{1}{A} \times \left(\frac{4Du - 2Cu^2}{8} + E\right)$.

We imagine that these cases are sufficient for shewing the management of the general equation; and the example of the numerical solution of the first case affords instances of the only niceties which occur in the process, viz. the proper employment of the positive and negative quantities.

We have oftener than once observed, that the formula is not perfectly accurate, and that in very large apertures errors will remain. It is proper therefore, when we have obtained the form of a compound object-glass, to calculate trigonometrically the progress of the light through it; and if we find a considerable aberration, either chromatic or spherical, remaining, we must make such changes in the curvatures as will correct them. We have done this for the first example; and we find, that if the focal distance of the compound object-glass be 100 inches, there remains of the spherical aberration nearly $\frac{1}{80}$th of an inch, and the aberration of colour is over-corrected above $\frac{1}{5}$th of an inch. The first aberration has been diminished about 6 times, and the other about 30 times. Both of the remaining errors will be diminished by increasing the radius of the inner surfaces. This will diminish the aberration of the crown-glass, and will diminish the dispersion of the flint more than that of the crown. But indeed the remaining error is hardly worth our notice.

It is evident to any person conversant with optical discussions, that we shall improve the correction of the spherical aberration by diminishing the refractions. If we employ two lenses for producing the convergency of the rays to a real focus, we shall reduce the aberration to $\frac{1}{4}$th. Therefore a better achromatic glass will be formed of three lenses, two of which are convex and of crown-glass. The refraction being thus divided between them, the aberrations are lessened. There is no occasion to employ two concave lenses of flint-glass; there is even an advantage in using one. The aberration being considerable, less of it will serve for correcting the aberration of the crown-glass, and therefore such a form may be selected as has little aberration. Some light is indeed lost by these two additional surfaces; but this is much more than compensated by the greater apertures which we can venture to give when the curvature of the surface is so much diminished. We proceed therefore to

The Construction of a Triple Achromatic Object-Glass.

It is plain that there are more conditions to be assumed before we can render this a determinate problem, and that the investigation must be more intricate. At the same time, it must give us a much greater variety of constructions, in consequence of our having more conditions necessary for giving the equation this determinate form. Our limits will not allow us to give a full account of all that may be done in this method. We shall therefore content ourselves with giving one case, which will sufficiently point out the method of proceeding. We shall then give the results in some other eligible cases, as rules to artists by which they may construct such glasses.

Let the first and second glasses be of equal curvatures on both sides; the first being a double convex of crown-glass, and the second a double concave of flint-glass.

Still making n the unit of our calculus, we have in the first place $a = -b, = -a', = b'$. Therefore $\frac{1}{a'} - \frac{1}{b'} = -\left(\frac{1}{a} - \frac{1}{b}\right)$, or $\frac{1}{n'} = -\frac{1}{n} = -1$. Therefore the equation $\frac{dm}{n} + \frac{dm'}{n'} + \frac{dm''}{n''} = 0$ becomes $u - 1 + \frac{u}{n''} = 0$, or $\frac{1}{n''} = \frac{1}{u} - 1$. Let us call this value u'.

We have $\frac{1}{p} = m - 1$; $\frac{1}{p'} = -(m' - 1)$; $\frac{1}{p''} = u'(m - 1)$; $\frac{1}{P} = \frac{1}{p} + \frac{1}{p'} + \frac{1}{p''}, = m - m' + u'(m - 1)$.

And if we make $m' - m = C'$, we shall have $\frac{1}{P} = - C + u'(m-1)$. Also $\frac{1}{r'} = m - 1$; $\frac{1}{r''} = m - 1 - (m-1), = m - m', = - C'$.

The equality of the two curvatures of each lens gives $\frac{1}{a} = \frac{1}{2n}$. Therefore $\frac{1}{a} = - \frac{1}{b}, = - \frac{1}{a'}, = \frac{1}{b'}, = \frac{1}{2}$; and $\frac{1}{b''} = \frac{1}{a''} - \frac{1}{n''}, = \frac{1}{a''} - u'$.

Substituting these values in the general equation, we obtain three formulæ,

$$1. \quad c m^2 - \tfrac{1}{2} c (2m+1) + \frac{c(m+2)}{4m}$$

$$2. \quad - m'^2 + \tfrac{1}{2}(2m'+1) - \frac{m'+2}{4m'} + (3m'+1)(m-1) - \frac{2(m'+1)(m-1)}{m'} - \frac{(3m'+2)(m-1)^2}{m'}$$

$$3. \quad c u'^3 m^2 - \frac{c u'^2 (2m+1)}{a''} + \frac{c w (m+2)}{m a''^2} - c c' u'^2 (3m+1) + \frac{4 c c' u' (m+1)}{m a''} + \frac{c c'^2 u' (3m+2)}{m} = 0.$$

Now arrange these quantities according as they are coefficients of $\frac{1}{a''^2}$ and of $\frac{1}{a''}$, or independent quantities. Let the coefficient of $\frac{1}{a''^2}$ be A, that of $\frac{1}{a''}$ be B, and the independent quantity be C, we have

$$A = \frac{c u' (m+2)}{m}; \; B = c u'^2 (2m+1) - \frac{4 c c' u' (m+1)}{m},$$

and $C = c m^2 + \frac{c(m+2)}{4m} + \frac{1}{2}(2m'+1) + (3m'+1)(m-1) + c u'^3 m^2 + \frac{c c'^2 u' (3m+2)}{m} - \frac{1}{2} c (2m+1)$

$$-m'^{2}-\frac{m'+2}{4m}-\frac{2(m'+1)(m-1)}{m'}=\frac{(3m'+2)(m-1)}{m'}$$

$-ec'u'_{-}(3m+1)$.

Our equation now becomes $\frac{A}{a''^{2}}-\frac{B}{a''}+C=0$.

This reduced to numbers, by computing the values of the coefficients, is $\frac{1,312}{a''^{2}}-\frac{1,207}{a''}-0,3257=0$.

This, divided by 1,312, gives $s=-0,92$; and $t=-0,2482$; $-\frac{1}{2}s=0,46$; $\frac{1}{4}s^{2}=0,2116$; and $\sqrt{\frac{1}{4}s^{2}-t}=\pm 0,6781$.

And, finally, $\frac{1}{a''}=0,46\pm 0,6781$.

This has two roots, viz. 0,2181 and — 1,1381. The last would give a very small radius, and is therefore rejected.

Now, proceeding with this value of $\frac{1}{a''}$ and the $\frac{1}{n''}$, we get the other radius b'', and then, by means of u'', we get the other radius, which is common to the four surfaces. Then, by $\frac{1}{P}=\frac{1}{p''}-c'$, we get the value of P.

The radii being all on the scale of which n is the unit, they must be divided by P to obtain their value on the scale which has P for its unit. This will give us

$$a=-b,=-a',=b',=0,530$$
$$a''=\quad 1,215$$
$$b''=\quad -0,3046$$
$$P=\quad 1.$$

This is not a very good form, because the last surface has too great curvature.

We thought it worth while to compute the curvatures for a case where the internal surfaces of the lenses coincide, in order to obtain the advantages mentioned on a former occasion. The form is as follows:

The middle lens is a double concave of flint-glass; the last lens is of crown-glass, and has equal curvatures on both sides. The following table contains the dimensions of the glasses for a variety of focal distances. The first column contains the focal distances in inches; the second contains the radii of the first surface in inches; the third contains the radii of the posterior surface of the first lens and anterior surface of the second; and the fourth column has the radii of the three remaining surfaces.

P	a	b, a'	b', a'', b''
12	9,25	6,17	12,75
24	18,33	12,25	25,5
36	27,33	18,25	38,17
48	36,42	24,33	50,92
60	45,42	30,33	63,58
72	54,5	36,42	76,33
84	63,5	42,5	89,
96	72,6	48,5	101,75
108	81,7	54,58	114,42
120	90,7	60,58	127,17

We have had an opportunity of trying glasses of this construction, and found them equal to any of the same length, although executed by an artist by no means excellent in his profession as a glass-grinder. This very circumstance gave us the opportunity of seeing the good effects of interposing a transparent substance between the glasses. We put some clear turpentine varnish between them, which completely prevented all reflection from the internal surfaces. Accordingly, these telescopes were surprisingly bright; and although the roughness left by the first grinding was very perceptible by the naked eye before the glasses were put together, yet when joined in this manner it entirely disappeared, even when the glasses were viewed with a deep magnifier.

The aperture of an object-glass of this construction of 30 inches focal distance was 3⅓th inches, which is considerably more than any of Mr Dollond's that we have seen.

If we should think it of advantage to make all the three lenses isosceles, that is, equally curved on both surfaces, the general equation will give the following radii:

$a = +\ 0{,}639 \qquad a' = -\ 0{,}5285 \qquad a'' = +\ 0{,}6413$

$b = -\ 0{,}639 \qquad b' = +\ 0{,}5285 \qquad b'' = -\ 0{,}6413$

This seems a good form, having large radii.

Should we choose to have the two crown-glass lenses isosceles and equal, we must make

$a = +\ 0{,}6412 \qquad a' = -\ 0{,}5227 \qquad a'' = +\ 0{,}6412$

$b = -\ 0{,}6412 \qquad b' = +\ 0{,}5367 \qquad b' = -\ 0{,}6412$

This form hardly differs from the last.

Our readers will recollect that all these forms proceed on certain measures of the refractive and dispersive powers of the substances employed, which are expressed by m, m', $d\,m$ and $d\,m'$: and we may be assured that the formulæ are sufficiently exact, by the comparison (which we have made in one of the cases) of the result of the formula and the trigonometrical calculation of the progress of the rays. The error was but $\frac{1}{60}$th of the whole, ten times less than another error, which unavoidably remains, and will be considered presently. These measures of refraction and dispersion were carefully taken; but there is great diversity, particularly in the flint-glass. We are well informed that the manufacture of this article has considerably changed of late years, and that it is in general less refractive and less dispersive than formerly. This must evidently make a change in the forms of achromatic glasses. The proportion of the focal distance of the crown-glasses to that of the flint must be increased, and this will occasion a change in the curvatures, which shall correct the spherical aberration. We examined with great care a parcel of flint-glass which an artist of this city got lately for the purpose of making achromatic object-glasses, and also some very white crown-glass made in Leith, and we obtained the following measures:

$$m = 1{,}529 \qquad \frac{dm}{dm'} = \frac{142}{219} = 0{,}64841.$$
$$m' = 1{,}578$$

We computed some forms for triple object-glasses made of these glasses, which we shall subjoin as a specimen of the variations which this change of data will occasion.

If all the three lenses are made isosceles, we have

$a = + 0{,}796 \qquad a' = - 0{,}474 \qquad a'' = + 0{,}502$
$b = - 0{,}796 \qquad b' = + 0{,}474 \qquad b'' = - 0{,}502$

Or

$a = 0{,}504 \qquad a' = - 0{,}475 \qquad a'' = + 0{,}793$
$b = - 0{,}504 \qquad b' = 0{,}475 \qquad b'' = - 0{,}793$

If the middle lens be isosceles, the two crown-glass lenses may be made of the same form and focal distance, and placed the same way. This will give us

$a = + 0{,}705 \qquad a' = - 0{,}475 \qquad a'' = + 0{,}705$
$b = - 0{,}547 \qquad b' = + 0{,}475 \qquad b'' = - 0{,}547$

N. B.—This construction allows a much better form, if the measures of refraction and dispersion are the same that we used formerly. For we shall have

$a = + 0{,}628 \qquad a' = - 0{,}579 \qquad a'' = + 0{,}628$
$b = - 0{,}749 \qquad b' = + 0{,}579 \qquad b'' = - 0{,}749$

And this is pretty near the practice of the London opticians.

We may here observe, upon the whole, that an amateur has little chance of succeeding in these attempts. The diversity of glasses, and the uncertainty of the workman's producing the very curvatures which he intends, is so great, that the object-glass turns out different from our expectation. The artist who makes great numbers acquires a pretty certain guess at the remaining error; and having many lenses, intended to be of one form, but unavoidably differing a little from it, he tries several of them with the other two, and finding one better than the rest, he makes use of it to complete the set.

The great difficulty in the construction is to find the exact proportion of the dispersive powers of the crown and flint glass. The crown is pretty constant; but there is hardly two pots of flint-glass which have the same dispersive power. Even if constant, it is difficult to measure it accurately; and an error in this greatly affects the instrument, because the focal distances of the lenses must be nearly as their dispersive powers. The method of examining this circumstance, which we found most accurate, was as follows:

The sun's light, or that of a brilliant lamp, passed through a small hole in a board, and fell on another board pierced also with a small hole. Behind this was placed a fine prism A (Fig. 15.), which formed a spectrum ROV on a screen pierced with a small hole. Behind this was placed a prism B of the substance under examination. The ray which was refracted by it fell on the wall at D, and the distance of its illumination from that point to C, on which an unrefracted ray would have fallen, was carefully measured. This showed the refraction of that colour. Then, in order that we might be certain that we always compared the refraction of the same precise colour by the different prisms placed at B, we marked the precise position of the prism A when the ray of a particular colour fell on the prism B. This was done by an index AG attached to A, and turning with it, when we caused the different colours of the spectrum formed by A to fall on B. Having examined one prism B with respect to all the colours in the spectrum formed by A, we put another B in its place. Then bringing A to all its former positions successively, by means of a graduated arch HGK, we were certain that when the index was at the same division of the arch it was the very ray which had been made to pass through the first prism B in a former experiment. We did not solicitously endeavour to find the very extreme red and violet rays; because, although we did not learn the whole dispersions of the two prisms, we learned their proportions, which is the circumstance wanted in the construction of achromatic glasses. It is in vain to attempt this by measuring the spectrums themselves; for we cannot be certain of selecting the very same colours for the comparison, because they succeed in an insensible gradation.

The intelligent reader will readily observe, that we have hitherto proceeded on the supposition, that when, by means of contrary refractions, we have united the extreme red and violet rays, we have also united all the others. But this is quite gratuitous. Sir Isaac Newton would, however, have made the same supposition; for he imagined that the different colours divided the spectrum formed by all substances in the proportions of a musical canon. This is a mistake. When a spectrum is formed by a prism of crown-glass, and another of precisely the same length is formed by the side of it by a prism of flint-glass, the confine between the green and blue will be found precisely in the middle of the first spectrum, but in the second it will be considerably nearer to the red extremity. In short, different substances do not disperse the colours in the same proportion.

The effect of this irrationality (so to call it) of dispersion, will appear plainly, we hope, in the following manner: Let A (Fig. 12.) represent a spot of white solar light falling perpendicularly on a wall. Suppose a prism of common glass placed behind the hole through which the light is admitted, with its refracting angle facing the left hand. It will refract the beam of light to the right, and will at the same time disperse this heterogeneous light into its component rays, carrying the extreme red ray from A to R, the extreme orange from A to O, the extreme yellow from A to Y, &c. and will form the usual prismatic spectrum ROYGBPVC. If the whole length RC be divided into 1000 parts, we shall have (when the whole refraction AR is small) RO very nearly 125, RY=200, RG=333, RB=500, RP=667, RV=778, and RC=1000; this being the proportion observed in the differences of the sines of refraction by Sir Isaac Newton.

Perhaps a refracting medium may be found such, that a prism made of it would refract the white light from A′, in the upper line of this figure, in such a manner that a spectrum R′O′Y′G′B′P′V′C′ shall be formed at the same distance from A′, and of the same length, but divided in a different proportion. We do not know that such a medium has been found; but we know that a prism of flint-glass has its refractive and dispersive powers so constituted, that if A′H′ be taken about ⅓d of AR, a spot of white light, formed by rays falling perpendicularly at H′, will be so refracted and dispersed, that the extreme red ray will be carried from H′ to R′, and the extreme violet from H′ to C′, and the intermediate colours to intermediate points, forming a spectrum resembling the other, but having the colours more constipated towards R′, and more dilated towards C; so that the ray which the common glass carried to the middle point B of the spectrum RC is now in a point B′ of the spectrum R′C′, considerably nearer to R′.

Dr Blair has found, on the other hand, that certain fluids, particularly such as contain the muriatic acid, when formed into a prism, will refract the light from H″ (in the lower line) so as to form a spectrum R″C″ equal to RC, and as far removed from A″ as RC is from A, but having the colours more dilated toward R″, and more constipated toward C, than is observed in RC; so that the ray which was carried by the prism of common glass to the middle point B is carried to a point B″, considerably nearer to C″.

Let us now suppose that, instead of a white spot at A, we have a prismatic spectrum AB (Fig. 18.), and that the prism of common glass is applied as before, immediately behind the prism which forms the spectrum AB. We know that this will be refracted sidewise, and will make a spectrum ROYGBPC, inclined to the plane of refraction in an angle of 45°; so that drawing the perpendicular RC′, we have RC′=*c*′ C.

We also know that the prism of flint-glass would refract the spectrum formed by the first prism on EHF, in such a manner that the red ray will go to R, the violet to C, and the intermediate rays to points *o*, *y*, *g*, *b*, *p*, *v*, so situated that O′ *o* is = R′O′ of the other figure; Y *y* is = R′Y′ of that figure, G *g* = R′G′, &c. These points must therefore lie in a curve R *o y g b p v* C, which is convex toward the axis R′C.′

In like manner we may be assured that Dr Blair's fluid will form a spectrum R *o′ y′ g′ b′ p′*, *v′* C, concave toward R′C′.

Let it be observed by the way, that this is a very good method for discovering whether a medium disperses the light in the same proportion with the prism which is employed for forming the first spectrum AB or EF. It disperses in the same or in a different proportion, according as the oblique spectrum is straight or crooked; and the exact proportion corresponding to each colour is had by measuring the ordinates of the curves R *b* C or R *b′* C.

Having formed the oblique spectrum RBC by a prism of common glass, we know that an equal prism of the same glass, placed in a contrary position, will bring back all the rays from the spectrum RBC to the spectrum AB, laying each colour on its former place.

In like manner, having formed the oblique spectrum R ′*b* C by a prism of flint-glass, we know that another prism of flint-glass, placed in the opposite direction, will bring all the rays back to the spectrum EHF.

But having formed the oblique spectrum RBC by a prism of common glass, if we place the flint-glass prism in the contrary position, it will bring the colour R back to E, and the colour C to F; but it will not bring the colour B to H, but to a point *h*, such that B *h* is equal to *b* H, and *b* B to *h* H. In like manner, the other colours will not be brought back to the straight line EHF, but to a curve E *h* F, forming a crooked spectrum.

In like manner, the fluids discovered by Dr Blair, when employed to bring back the oblique spectrum RBC formed by common glass, will bring its extremities back to E and F, and form the crooked spectrum E *h'* F lying beyond EHF.

This experiment evidently gives us another method for examining the proportionality of the dispersion of different substances.

Having, by common glass, brought back the oblique spectrum formed by common glass to its natural place AB, suppose the original spectrum at AB to contract gradually (as Newton has made it do by means of a lens), it is plain that the oblique spectrum will also contract, and so will the second spectrum at A B; and it will at last coalesce into a white spot. The effect will be equivalent to a gradual compression of the whole figure, by which the parallel lines AR and BC gradually approach, and at last unite.

In like manner, when the oblique spectrum formed by flint-glass is brought back to EHF by a flint-glass prism, and the figure compressed in the same gradual manner, all the colours will coalesce into a white spot.

But when flint-glass is employed to bring back the oblique spectrum formed by common glass, it forms the crooked spectrum E *h* F. Now let the figure be compressed. The curve E *h* F will be doubled down on the line H *h*, and there will be formed a compound spectrum H *h*, quite unlike the common spectrum, being purple or claret coloured at H by the mixture of the extreme red and violet, and green edged with blue at *h* by the mixture of the green and blue. The fluid prisms would in like manner form a spectrum of the same kind on the other side of H.

This is precisely what is observed in achromatic object-glasses made of crown-glass and flint: for the refraction from A to R corresponds to the refraction of the convex crown-glass; and the contrary refraction from R to E corresponds to the contrary refraction of the concave flint-glass, which still leaves a part of the first refraction, producing a convergence to the axis of the telescope. It is found to give a purple or wine-coloured focus, and within this a green one, and between these an imperfect white. Dr Blair found, that when the eye-glass was drawn out beyond its proper distance, a star was surrounded by a green fringe, by the green end of the spectrum, which crossed each other within the focus; and when the eye-glass was too near the object-glass, the star had a wine-coloured fringe. The green rays were ultimately most refracted. *N. B.* We should expect the fringe to be of a blue colour rather than a green. But this is easily explained: The extreme violet rays are very faint, so as hardly to be sensible; therefore when a compound glass is made as achromatic as possible to our senses, in all probability (nay certainly) these almost insensible violet rays are left out, and perhaps the extreme colours which are united are the red and the middle violet rays. This makes the green to be the mean ray, and therefore the most outstanding when the dispersions are not proportional.

Dr Blair very properly calls these spectrums, H *h* and H *h′*, *secondary spectrums*, and seems to think that he is the first who has taken notice of them. But Mr Clairault was too accurate a mathematician, and too careful an observer, not to be aware of a circumstance which was of primary consequence to the whole inquiry. He could not but observe that the success rested on this very particular, and that the proportionality of dispersion was indispensably necessary.

This subject was therefore touched on by Clairault; and *fully* discussed by Boscovich, first in his Dissertations published at Vienna in 1759; then in the *Comment. Bononiensis*; and, lastly, in his *Opuscula*, published in 1785. Dr Blair, in his ingenious Dissertation on Achromatic Glasses, read to the Royal Society of Edinburgh in 1793, seems not to have known of the labours of these writers;

speaks of it as a new discovery; and exhibits some of the consequences of this principle in a singular point of view, as something very paradoxical and inconsistent with the usually received notions on these subjects. But they are by no means so. We are, however, much indebted to his ingenious researches, and his successful endeavours to find some remedy for this imperfection of achromatic glasses. Some of his contrivances are exceedingly ingenious; but had the Doctor consulted these writers, he would have saved himself a good deal of trouble.

Boscovich shows how to unite the two extremes with the most outstanding colour of the secondary spectrum, by means of a third substance. When we have done this, the aberration occasioned by the secondary spectrums must be prodigiously diminished; for it is evidently equivalent to the union of the points H and *h* of our figure. Whatever cause produces this must diminish the curvature of the arches E *h* and *h* F: but even if these curvatures were not diminished, their greatest ordinates cannot exceed ¼th of H *h*; and we may say, without hesitation, that by uniting the mean or most outstanding ray with the two extremes, the remaining dispersion will be as much less than the uncorrected colour of Dollond's achromatic glass, as this is less than four times the dispersion of a common object-glass. It must therefore be altogether insensible.

Boscovich asserts, that it is not possible to unite more than two colours by the opposite refraction of two substances, which do not disperse the light in the same proportions. Dr Blair makes light of this assertion, as he finds it made in general terms in the extract made by Priestley from Boscovich in his Essay on the History of Optics; but had he read this author in his own dissertations, he would have seen that he was perfectly right. Dr Blair, however, has hit on a very ingenious and effectual method of producing this union of three colours. In the same way as we correct the dispersion of a concave lens of crown-glass by the opposite dispersion of a concave lens of flint-glass, we may correct the secondary dispersion of an achromatic convex lens by the opposite secondary dispersion of an achromatic concave lens. But the intelligent reader will observe, that this union does not contradict the assertion of Boscovich, because it is *necessarily* produced by means of three refracting substances.

The most essential service which the public has received at the hands of Dr Blair is the discovery of fluid mediums of a proper dispersive power. By composing the lenses of such substances, we are at once freed from the irregularities in the refraction and dispersion of flint-glass, which the chemists have not been able to free it from. In whatever way this glass is made, it consists of parts which differ both in refractive and dispersive power; and when taken up from the pot, these parts mix in threads, which may be disseminated through the mass in any degree of fineness. But they still retain their properties; and when a piece of flint-glass has been formed into a lens, the eye, placed in its focus, sees the whole surface occupied by glistening threads or broader veins running across it. Great rewards have been offered for removing this defect, but hitherto to no purpose. We beg leave to propose the following method: Let the glass be reduced to powder, and then melted with a great proportion of alkaline salt, so as to make a liquor silicum. When precipitated from this by an acid, it must be in a state of very uniform composition. If again melted into glass, we should hope that it would be free from this defect; if not, the case seems to be desperate.

But by using a fluid medium, Dr Blair was freed from all this embarrassment; and he acquired another immense advantage, that of adjusting at pleasure both the refractive and dispersive powers of his lenses. In solid lenses, we do not know whether we have taken the curvatures suited to the refractions till our glass is finished; and if we have mistaken the proportions, all our labour is lost. But when fluids are used, it is enough that we know nearly the refractions. We suit our focal distances to these, and then select our curvatures, so as to remove the aberration of figure, preserving the focal distances. Thus, by properly tempering the fluid mediums, we bring the lens to agree precisely with the theory, perfectly achromatic, and the aberration of figure as much corrected as is possible.

Dr Blair examined the refractive and dispersive powers of a great variety of substances, and found great varieties in their actions on the different colours.* This is indeed what every well-informed naturalist would expect. There is no doubt now among naturalists about the mechanical connexion of the phenomena of nature; and all are agreed that the chemical actions of the particles of matter are perfectly like in kind to the action of gravitating bodies; that all these phenomena are the effects of forces like those which we call attractions and repulsions, and which we observe in magnets and electrified bodies; that light is refracted by forces of the same kind, but differing chiefly in the small extent of their sphere of activity. One who views things in this way will expect, that as the actions of the same acid for the different alkalis are different in degree, and as the different acids have also different actions on the same alkali, in like manner different substances differ in their general refractive powers, and also in the proportion of their action on the different colours. Nothing is more unlikely therefore than the proportional dispersion of the different colours by different substances; and it is surprising that this inquiry has been so long delayed. It is hoped that Dr Blair will oblige the public with an account of the experiments which he has made. This will enable others to co-operate in the improvement of achromatic glasses. We cannot derive much knowledge from what he has already published, because it was chiefly with the intention of giving a popular, though not an accurate view of the subject. The constructions which are there mentioned are not those which he found most effectual, but those which would be most easily understood, or demonstrated by the slight theory which is contained in the dissertation; besides, the manner of expressing the difference of refrangibility, perhaps chosen for its paradoxical appearance, does not give us a clear notion of the characteristic differences of the substances examined. Those rays which are ultimately most deflected from their direction, are said to have become the most refrangible by the combination of different substances, although, in all the particular refractions by which this effect is produced, they are less refracted than the violet light. We can just gather this much, that common glass disperses the rays in such a manner, that the ray which is in the confine of the green and blue occupies the middle of the prismatic spectrum; but in glasses, and many other substances, which are more dispersive, this ray is nearer to the ruddy extremity of the spectrum. While therefore the straight line RC′ (Fig. 13.) terminates the ordinates O *o*′, YY′, G *g*′, &c. which represent the dispersion of common glass, the ordinates which express the dispersions of these substances are terminated by a curve passing through R and C′, but lying below the line RC′. When therefore parallel heterogeneous light is made to converge to the axis of a convex lens of common glass, as happens at F in Fig. 8. the light is dispersed, and the violet rays have a shorter focal distance. If we now apply a concave lens of greater dispersive power, the red and violet rays are brought to one focus F′; but the green rays, not being so much refracted away from F, are left behind at φ, and have now a shorter focal distance. But Dr Blair afterwards found that this was not the case with the muriatic acid, and some solutions in it. He found that the ray which common glass caused to occupy the middle of the spectrum was much nearer to the blue extremity when refracted by these fluids. Therefore a concave lens formed of such fluids which united the red and violet rays in F', refracted the green rays to f'.

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* A very full series of experiments on this subject will be found in Dr Brewster's *Treatise on new Philosophical Instruments*, p. 315, and in the *Edinburgh Transactions*, vol. VIII. p. 1.

Having observed this, it was an obvious conjecture, that a mixture of some of these fluids might produce a medium, whose action on the intermediate rays should have the same proportion that is observed on common glass; or that two of them might be found which formed spectra similarly divided, and yet differing sufficiently in dispersive power to enable us to destroy the dispersion by contrary refractions, without destroying the whole refraction. Dr Blair accordingly found a mixture of solutions of ammoniacal and mercurial salts, and also some other substances, which produced dispersions proportional to that of glass, with respect to the different colours.

And thus has the result of this intricate and laborious investigation corresponded to his utmost wishes. He has produced achromatic telescopes which seem as perfect as the thing will admit of; for he has been able to give them such apertures, that the *incorrigible* aberration arising from the spherical surfaces becomes a sensible quantity; and precludes farther amplification by the eye-glasses. We have examined one of his telescopes: The focal distance of the object-glass did not exceed 17 inches, and the aperture was fully 3½ inches. We viewed some single and double stars and some common objects with this telescope; and found, that in magnifying power, brightness, and distinctness, it was manifestly superior to one of Mr Dollond's of 42 inches focal length. It also gave us an opportunity of admiring the dexterity of the London artists, who could work the glasses with such accuracy. We had most distinct vision of a star when using an erecting eye-piece, which made this telescope magnify more than a hundred times; and we found the field of vision as uniformly distinct as with Dollond's 42 inch telescope magnifying 46 times. The intelligent reader must admire the nice figuring and centering of the very deep eye-glasses which are necessary for this amplification.

It is to be hoped that Dr Blair will extend his views to *glasses* of different compositions, and thus give us object-glasses which are solid; for those composed of fluids have inconveniences which will hinder them from coming into general use, and will confine them to the museums of philosophers. We imagine that antimonial glasses bid fair to answer this purpose, if they could be made free of colour, so as to transmit enough of light. We recommend this dissertation to the careful perusal of our readers. Those who have not made themselves much acquainted with the delicate and abstruse theory of aberrations, will find it exhibited in such a popular form as will enable them to understand its general aim; and the well-informed reader will find many curious indications of inquiries and discoveries yet to be made.

We now proceed to consider the eye-glasses or glasses of telescopes. The proper construction of an eye-piece is not less essential than that of the object-glass. But our limits will not allow us to treat this subject in the same detail. We have already extended this article to a great length, because we do not know of any performance in the English language which will enable our readers to understand the construction of achromatic telescopes; an invention which reflects honour on our country, and has completed the discoveries of our illustrious Newton. Our readers will find abundant information in Dr Smith's Optics concerning the eye-glasses, chiefly deduced from Huyghens's fine theory of aberration.† At the same time, we must again pay Mr

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† While we thus repeatedly speak of the theory of spherical aberration as coming from Mr Huyghens, we must not omit giving a due

Dollond the merited compliment of saying, that he was the first who made any scientific application of this theory to the compound eye-piece for erecting the object. His eye-pieces of five and six glasses are very ingenious reduplications of Huyghens's eye-piece of two glasses, and would probably have superseded all others, had not his discovery of achromatic object-glasses caused opticians to consider the chromatic dispersion with more attention, and pointed out methods of correcting it in the eye-piece without any compound eye-glasses. They have found that this may be more conveniently done with four eye-glasses, without sensibly diminishing the advantages which Huyghens showed to result from employing many small refractions instead of a lesser number of great ones. As this is a very curious subject, we shall give enough for making our readers fully acquainted with it, and content ourselves with merely mentioning the principles of the other rules for constructing an eye-piece.

Such readers as are less familiarly acquainted with optical discussions will do well to keep in mind the following consequences of the general focal theorem:

If AB (Fig. 14.) be a lens, R a radiant point or focus of incident rays, and *a* the focus of parallel rays coming from the opposite side; then,

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share of the honour of it to Dr Barrow and Mr James Gregory. The first of these authors, in his Optical Lectures delivered at Cambridge, has given every proposition which is employed by Huyghens, and has even prosecuted the matter much further. In particular, his theory of oblique slender pencils is of immense consequence to the perfection of telescopes, by showing the methods for making the image of an extended surface as flat as possible. Gregory, too, has given all the fundamental propositions in his *Optica Promota*. But Huyghens, by taking the subject together, and treating it in a system, has greatly simplified it: and his manner of viewing the principal parts of it is incomparably more perspicuous than the performances of Barrow and Gregory.

1. Draw the perpendicular *a a'* to the axis, meeting the incident ray in *a'*, and *a'* A to the centre of the lens. The refracted ray BF is parallel to *a'* A: for R *a'*: *a'* A (= R *a* : *a* A) = RB : BF (= RA : AF), which is the focal theorem.

2. An oblique pencil BP *b* proceeding from any point P which is not in the axis, is collected to the point *f*, where the refracted ray BF cuts the line PA*f* drawn from P through the centre of the lens: for P *a'*; *a'* A=PB : B*f*, which is also the focal theorm.

The Galilean telescope is susceptible of so little improvement, that we need not employ any time in illustrating its performance.

The simple astronomical telescope is represented in Fig. 16. The beam of parallel rays, inclined to the axis, is made to converge to a point G, where it forms an image of the lowest point of a very distant object. These rays decussating from G fall on the eye-glass; the ray from the lowest point B of the object-glass falls on the eye-glass at *b*; and the ray from A falls on *a*; and the ray from the centre O falls on *o*. These rays are rendered parallel, or nearly so, by refraction through the eye-glass, and take the direction *b i'*, *o* I, *a i*. If the eye be placed so that this pencil of parellel rays may enter it, they converge to a point of the retina, and give distinct vision of the lowest point of the object. It appears inverted, because the rays by which we see its lowest point come in the direction which, in simple vision, is connected with the upper point of an object. They come from above, and therefore are thought to proceed from above. We see the point as if situated in the direction I *o*. In like manner the eye placed at I, sees the upper point of the object in the direction IP, and its middle in the direction IE. The proper place for the eye is I: if brought much nearer the glass, or removed much farther from it, some, or the whole, of this extreme pencil of rays will not enter the pupil. It is therefore of importance to determine this point. Because the eye requires parallel rays for distinct vision, it is plain that F must be the principal focus of the eye-glass. Therefore, by the common focal theorem OF : OE = OE : OI, or OF : FE = OE : EI.

The magnifying power being measured by the magnitude of the visual angle, compared with the magnitude of the visual angle with the naked eye, we have $\frac{o\,\mathrm{I}\,p}{o\,\mathrm{O}\,p}$, or $\frac{o\,\mathrm{IF}}{o\mathrm{OF}}$ for the measure of the magnifying power. This is very nearly $= \frac{\mathrm{OE}}{\mathrm{EI}}$, or $\frac{\mathrm{OF}}{\mathrm{FI}}$.

As the line OE, joining the centres of the lenses, and perpendicular to their surfaces, is called the axis of the telescope, so the ray OG is called the axis of the oblique pencil, being really the axis of the cone of light which has the object-glass for its base. This ray is through its whole course the axis of the oblique pencil; and when its course is determined, the amplification, the field of vision, the apertures of the glasses, are all determined. For this purpose we have only to consider the centre of the object-glass as a radical point, and trace the process of a ray from this point through the other glasses: this will be the axis of some oblique pencil.

It is evident, therefore, that the field of vision depends on the breadth of the eye-glass. Should we increase this, the extreme pencil will pass through I, because O and I are still the conjugate foci of the eye-glass. On the other hand, the angle resolved on for the extent or field of vision gives the breadth of the eye-glass.

We may here observe, by the way, that for all optical instruments there must be two optical figures considered. The first shews the progress of a pencil of rays coming from one point of the object. The various focusses of this pencil show the places of the different images, real or virtual. Such a figure is formed by the three rays AG *a i′*, OG *o* I, BG *b i*.

The second shows the progress of the axes of the different pencils proceeding through the centre of the object-glass. The focusses of this pencil of axes show the places where an image of the object-glass is formed; and this pencil determines the field of vision, the apertures of the lenses, and the amplification or magnifying power. The three rays OG *o* I, OFEI, OHPI, form this figure.

See also fig. 24. where the progress of both sets of pencils is more diversified.

The perfection of a telescope is to represent an object in its proper shape, distinctly magnified, with a great field of vision, and sufficiently bright. But there are limits to all these qualities; and an increase of one of them, for the most part, diminishes the rest. The brightness depends on the aperture of the object-glass, and will increase in the same proportion (because *i i'* will always be to AB in the proportion of EF to FO), till the diameter of the emergent pencil is equal to that of the pupil of the eye. Increasing the object-glass any more can send no more light into the eye. But we cannot make the emergent pencil nearly so large as this when the telescope magnifies much; for the great aperture of the object-glass produces an indistinct image at GF, and indistinctness is magnified by the eye-glass.

A great field of vision is incompatible with the true shape of the object; for it is not strictly true that all rays flowing from O are refracted to I. Those rays which go to the margin of the eye-glass cross the axis between E and I; and therefore they cross it at a greater angle than if they passed through I. Now had they really passed through I, the object would have been represented in its due proportions. Therefore since the angles of the marginal parts are enlarged by the aberration of the eye-glass, the marginal parts themselves will appear enlarged, or the object appear distorted. Thus, a chess-board viewed through a reading-glass appears drawn out at the corners, and the straight lines are all changed into curves, as is represented in fig. 18.

The circumstance which most peremptorily limits the extent of field is the necessary distinctness. If the vision be indistinct, it is useless, and no other quality can compensate this defect. The distortion is very inconsiderable in much larger angles of vision than we can admit, and is unworthy of the attention paid to it by optical writers. They have been induced to take notice of it, because the means of correcting it in a considerable degree are attainable, and afford an opportunity of exhibiting their knowledge; whereas the indistinctness which accompanies a large field is a subjcet of most difficult discussion, and has hitherto baffled all their efforts to express by any intelligible or manageable formulæ.

This subject must, however, be considered. The image at GF of a very remote object is not a plain surface perpendicular to the axis of the telescope, but is nearly spherical, having O for its centre. If a number of pencils of parallel rays crossing each other in I fall on the eye-glass, they will form a picture on the opposite side, in the focus F. But this picture will by no means be flat, nor nearly so, but very concave towards E. Its exact form is of most difficult investigation. The elements of it are given by Dr Barrow; and we have given the chief of them in the article Optics, when considering the foci of infinitely slender pencils of oblique rays. Therefore it is impossible that the picture formed by the object-glass can be seen distinctly in all its parts by the eye-glass. Even if it were flat, the points G and H (Fig. 16.) are too far from the eye-glass when the middle F is at the proper distance for distinct vision. When, therefore, the telescope is so adjusted that we have distinct vision of the middle of the field, in order to see the margin distinctly we must push in the eye-glass: and having so done, the middle of the field becomes indistinct. When the field of vision exceeds 12 or 15 degrees, it is not possible by any contrivance to make it tolerably distinct all over; and we must turn the telescope successively to the different parts of the field that we may see them agreeably.

The cause of this indistinctness is, as we have already said, the shortness of the lateral foci of lateral and oblique pencils refracted by the eye-glass. The common determination of these is not complete, and relates only to those rays which are on a plane passing through the axis of the lens. But the oblique pencil *b* G *a*, by which an eye placed at I sees the point G of the image, is a cone of light, having a circular base on the eye-glass; of which circle *a b* is one of the diameters. There is a diameter perpendicular to this, which, in this figure, is represented by the point *o*. Fig. 17. represents the base of the cone as seen by an eye placed in the axis of the telescope, with the object-glass as appearing behind it. The point *b* is formed by a ray which comes from the lowest point B of the object-glass, and the point *a* is illuminated by a ray from A. The point *c* at the right-hand of the circular base of this cone of light came from the point C on the left side of the object-glass; and the light comes to *d* from D. Now the laws of optics demonstrate, that the rays which come through the points *c* and *d* are more convergent after refraction than the rays which come through *a* and *b*. The analogies, therefore, which ascertain the foci of rays lying in the planes passing through the axis do not determine the foci of the others. Of this we may be sensible by looking through a lens to a figure on which are drawn concentric circles crossed by radii. When the telescope is so adjusted that we see distinctly the extremity of one of the radii, we shall not see distinctly the circumference which crosses the extremity with equal distinctness, and *vice versa*. This difference, however, between the foci of the rays which come through *a* and *b*, and those which come through *c* and *d*, is not considerable in the fields of vision, which are otherwise admissible. But the same difference of foci obtains also with respect to the dispersion of light, and is more remarkable. Both d'Alembert and Euler have attempted to introduce it into their formulæ; but they have made them useless for any practical purpose by their inextricable complication.

This must serve as a general indication of the difficulties which occur in the construction of telescopes, even although the object-glass were perfect, forming an image without the smallest confusion or distortion.

There is yet another difficulty or imperfection. The rays of the pencil *a* G *b*, when refracted through the eye-glass, are also separated into their component colours. The edge of the lens must evidently perform the office of a prism, and the white ray G *b* will be so dispersed that if $b\,i'$ be the path of its red ray, the violet ray, which makes another part of it, will take such a course $b\,n$ that the angle $i'\,b\,n$ will be nearly $\frac{1}{27}$th of G $b\,i'$. The ray G *a* passing through a part of the lens whose surfaces are less inclined to each other, will be less refracted, and will be less dispersed in the same proportion very nearly. Therefore the two violet rays will be very nearly parallel when the two red rays are rendered parallel.

Hence it must happen, that the object will appear bordered with coloured fringes. A black line seen near the margin on a white ground will have a ruddy and orange border on the outside and a blue border within: and this confusion is altogether independent on the object-glass and is so much the greater as the visual angle *b* IE is greater.

Such are the difficulties: They would be insurmountable were it not that some of them are so connected that, to a certain extent, the diminution of one is accompanied by a diminution of the other. These curves are the geometrical loci of the foci of infinitely slender pencils. Consequently the point G is very nearly in the caustic formed by a beam of light consisting of rays parallel to I *o*, and occupying the whole surface of the eye-glass, because the pencil of rays which are collected at G is very small. Any thing therefore that diminishes the mutual inclination of the adjoining rays puts their concourse farther off. Now this is precisely what we want: for the point G of the image formed by the object-glass is already beyond the focus of the oblique slender pencil of parallel rays *i a* and *i b*; and, therefore, if we could make this focus go a little farther from *a* and *b*, we shall bring it nearer to G, and obtain more distinct vision of this point of the object. Now let it be recollected, that in moderate refractions through prisms, two rays which are inclined to each other in a small angle are, after refraction, inclined to each other in the same angle. Therefore, if we can diminish the aberration of the ray *a i*, or *o* I, or *b i′*, we diminish their mutual inclination; and consequently the mutual inclination of the rays G *a*, G *o*, G *b′*, and therefore lengthen the focus, and get more distinct vision of the point G. Therefore we at once correct the distortion and the indistinctness: and this is the aim of Mr Huyghens's great principle of dividing the refractions.

The general method is as follows: Let *o* be the object-glass (Fig. 19.) and E the eye-glass of a telescope, and F their common focus, and FG the image formed by the object-glass. The proportion of their focal distances is supposed to be such as gives as great a magnifying power as the perfection of the object-glass will admit. Let BI be the axis of the emergent pencil. It is known by the focal theorem that GE is parallel to BI: therefore BGE is the whole refraction or deflection of the ray OHB from its former direction. Let it be proposed to diminish the aberrations by dividing this into two parts by means of two glasses D and *e*, so as to make the ultimate angle of vision *b i e* equal to BIE, and thus retain the same magnifying power and visible field. Let it be proposed to divide it into the parts BGC and CGE.

From G draw any line GD to the axis towards O; and draw the perpendicular DH, cutting OG in H; draw H *c* parallel to GC, cutting GD in *g*; draw *g f* perpendicular to the axis; and *g e* parallel to GE; draw *e b* perpendicular to the axis; draw D δ parallel to GC, and δ *d* perpendicular to the axis.

Then if there be placed at D a lens whose focal distance is D *d*, and another at *e*, whose focal distance is *e f*, the thing is done. The ray OH will be refracted into H *b*, this into *b i* parallel to BI.

The demonstration of this construction is so evident by means of the common focal theorem, that we need not repeat it, nor the reason for its advantages. We have the same magnifying power, and the same field of vision; we have less aberration, and therefore less distortion and indistinctness; and this is brought about by a lens HD of a smaller aperture and a greater focal distance than BE. Consequently, if we are contented with the distinctness of the margin of the field with a single eye-glass, we may greatly increase the field of vision: for if we increase DH to the size of EB we shall have a greater field, and much greater distinctness in the margin; because HD is of a longer focal distance, and will bear a greater aperture, preserving the same distinctness at the edge. On this account the glass HD is commonly called the *Field-glass.*

It must be observed here, however, that although the distortion of the object is lessened, there is a real distortion produced in the image *f g*. But this, when magnified by the glass *e*, is smaller than the distortion produced by the glass E, of greater aperture and shorter focus, on the undistorted image GF. But because there is a distortion in the second image *f g*, this construction cannot be used for the telescopes of astronomical quadrants, and other graduated instruments; because then equal divisions of the micrometer would not correspond to equal angles.

But the same construction will answer in this case, by taking the point D on that side of F which is remote from

O (Fig. 20.) This is the form now employed in the telescopes of all graduated instruments.

The exact proportion in which the distortion and the indistinctness at the edges of the field are diminished by this construction, depends on the proportion in which the angle BGE is divided by GC; and is of pretty difficult investigation. But it never deviates far (never $\frac{1}{8}$ in optical instruments) from the proportion of the squares of the angles. We may, without any sensible error, suppose it in this proportion. This gives us a practical rule of easy recollection, and of most extensive use. When we would diminish an aberration by dividing the whole refraction into two parts, we shall do it most effectually by making them equal. In like manner, if we divide it into three parts by means of two additional glasses, we must make each $=\frac{1}{3}$d of the whole; and so on for a greater number.

This useful problem, even when limited, as we have done, to equal refractions, is as yet indeterminate; that is, susceptible of an infinity of solutions: for the point D, where the field-glass is placed, was taken at pleasure; yet there must be situations more proper than others. The aberrations which produce distortion, and those which produce indistinctness, do not follow the same proportions. To correct the indistinctness, we should not select such positions of the lens HD as will give a small focal distance to *b e;* that is, we should not remove it very far from F. Huyghens recommends the proportion of 3 to 1 for that of the focal distances of the lens HD and *e b*, and says that the distance D *e* should be $= 2$ F *e*. This puts *f* too near to HD, and thus shows the dust on HD. This will make $e\,i = \frac{1}{2}\,e\,F$, and will divide the whole refraction into two equal parts, as any one will readily see by constructing the common optical figure. Mr Short, the celebrated improver of reflecting telescopes, generally employed this proportion; and we shall presently see that it is a very good one.

It has been already observed, that the great refractions which take place on the eye-glasses occasion very considerable dispersions, and disturb the vision by fringing every thing with colours. To remedy this, achromatic eye-glasses may be employed, constructed by the rules already delivered. This construction, however, is incomparably more intricate than that of object-glasses: for the equations must involve the distance of the radiant point, and be more complicated: and this complication is immensely increased on account of the great obliquity of the pencils.

Most fortunately the Huyghenian construction of an eyepiece enables us to correct this dispersion to a great degree of exactness. A heterogeneous ray is dispersed at H, and the red ray belonging to it falls on the lens *b e* at a greater distance from the centre than the violet ray coming from H. It will therefore be less refracted (*cæteris paribus*) by the lens *b e*; and it is possible that the difference may be such that the red and violet rays dispersed at H may be rendered parallel at *b*, or even a little divergent, so as to unite accurately with the red ray at the bottom of the eye. How this may be effected, by a proper selection of the places and figures of the lenses, will appear by the following proposition, which we imagine is new and not inelegant:

Let the compound ray OP (Fig. 21) be dispersed by the lens PC; and let PV, PR be its violet and red rays, cutting the axis in G and *g*. It is required to place another lens RD in their way, so that the emergent rays R *r*, V *v*, shall be parallel.

Produce the incident ray OP to Z. The angles ZPR, ZPV, are given, (and RPV is nearly $=\frac{\text{ZPR}}{27}$) and the intersections G and *g* with the axis. Let F be the focus of parallel red light coming through the lens RD in the opposite direction. Then (by the common optical theorem), the perpendicular F *ε* will cut PR in such a point *ε*, that *ε* F will be parallel to the emergent ray R *r* and to V *v*.

Therefore if ρ D cut PV in u, and uf be drawn perpendicular to the axis, we shall have (also by the common theorem) the point f for the focus of violet rays, and DF : Df = Dρ : Du = 28 : 27 nearly, in a given ratio.

The problem is therefore reduced to this: "To draw from a point D in the line CG a line Dρ, which shall be cut by the lines PR and PV in the given ratio.

The following construction naturally offers itself: Make GM : g M in the given ratio, and draw MK parallel to Pg. Through *any* point D of CG draw the straight line PDK, cutting MK in K. Join GK, and draw Dρ parallel to KG. This will solve the problem; and, drawing ρ F perpendicular to the axis, we shall have F for the focus of the lens RD for parallel red rays.

The demonstration is evident; for MK being parallel to Pg, we have GM : g M = GK : HK, = ρ D, : u D = FD : fD, in the ratio required.

This problem admits of an infinity of solutions; because the point D may be taken any where in the line CG. It may therefore be subjected to such conditions as may produce other advantages.

1. It may be restricted by the magnifying power, or by the division which we choose to make of the whole refraction which produces this magnifying power. Thus, if we have resolved to diminish the aberrations by making the two refractions equal, we have determined the angle RrD. Therefore draw GK, making the angle MGK equal to that which the emergent pencil must make with the axis, in order to produce this magnifying power. Then draw MK parallel to Pg, meeting GK in K. Then draw PK, cutting the axis in D, and Dρ parallel to GK, and ρ F perpendicular to the axis. D is the place, and DF the focal distance of the eye-glass.

2. Particular circumstances may cause us to fix on a particular place D, and we only want the focal distance. In this case the first construction suffices.

3. We may have determined on a certain focal distance DF, and the place must be determined. In this case let

$$GF : F\epsilon = 1 : \text{tan. } G$$
$$F\epsilon : fu = 1 : m, \ m \text{ being} = \tfrac{27}{28}$$
$$fu : fg = \text{tan. } g : 1$$

then $GF : fg = \text{tan. } g : m \text{ tan. } G$

then $GF - fg : GF = \text{tan. } g - m \text{ tan. } G : \text{tan. } g$

or $Gg + Ff : GF = \text{tan. } g - m \text{ tan. } G : \text{tan. } g$;

and $GF = Gg + Ff \dfrac{\text{tan. } g}{\text{tan. } g - m. \text{ tan. } G}$, and is therefore given, and the place of F is determined; and since FD is given by supposition, D is determined.

The application of this problem to our purpose is difficult, if we take it in the most general terms; but the nature of the thing makes such limitations that it becomes very easy. In the case of the dispersion of light, the angle GPg is so small that MK may be drawn parallel to PG without any sensible error. If the ray OP were parallel to CG, then G would be the focus of the lens PC, and the point M would fall on C; because the focal distance of red rays is to that of violet rays in the same proportion for every lens, and therefore CG : Cg = DF : Df. Now, in a telescope which magnifies considerably, the angle at the object-glass is very small, and CG hardly exceeds the focal distance; and CG is to Cg very nearly in the same proportion of 28 to 27. We may therefore draw through C (Fig. 21.) a line CK parallel to PG: then draw GK′ perpendicular to the axis of the lenses, and join PK′; draw K′BE parallel to CG, cutting PK in B; draw BHI parallel to GK, cutting GK′ in H: Join HD and PK. It is evident that CG is bisected in F′, and that K′B = 2 F′D: also K′H : HG = KB : BE, = CD : DG. Therefore DH is parallel to CK′, or to PG. But because PF′ = F′K′, I′D is = DB, and IH = HB. Therefore ′D = HB, and

$FD = K'B, = 2F'D$; and FD is bisected in F′. Therefore $CD = \frac{CG+FD}{2}$.

That is, in order that the eye-glass RD may correct the dispersion of the field-glass PC, *the distance between them must be equal to the half sum of their focal distances* very nearly. More exactly, *the distance between them must be equal to the half sum of the focal distance of the eye-glass, and the distance at which the field-glass would form an image of the object-glass.* For the point G is the focus to which a ray coming from the centre of the object-glass is refracted by the field-glass.

This is a very simple solution of this important problem. Huyghens's eye-piece corresponds with it exactly. If indeed the dispersion at P is not entirely produced by the refraction, but perhaps combined with some previous dispersion, the point M (Fig. 21.) will not coincide with C, (Fig. 22.), and we shall have GC to GM, as the natural dispersion at P to the dispersion which really obtains there. This may destroy the equation $= \frac{CG+FD}{2}$.

Thus, in a manner rather unexpected, have we freed the eye-glasses from the greatest part of the effect of dispersion. We may do it entirely by pushing the eye-glass a little nearer to the field-glass. This will render the violet rays a little divergent from the red, so as to produce a perfect picture at the bottom of the eye. But by doing so we have hurt the distinctness of the whole picture, because F is not in the focus of RD. We remedy this by drawing both glasses out a little, and the telescope is made perfect.

This improvement cannot be applied to the construction of quadrant telescopes, such as Fig. 20. Mr Ramsden has attempted it, however, in a very ingenious way, which merits a place here, and is also instructive in another way. The field-glass HD (Fig. 20.) is a plano-convex, with its plane side next the image GF. It is placed very near this image. The consequence of this disposition is, that the image GF produces a vertical image *g f*, which is much less convex towards the glass. He then places a lens on the point C, where the red ray would cross the axis. The violet ray will pass on the other side of it. If the focal distance of this glass be *f c*, the vision will be distinct and free from colour. It has, however, the inconveniency of obliging the eye to be close to the glass, which is very troublesome.

This would be a good construction for a magic-lantern, or for the object-glass of a solar microscope, or indeed of any compound microscope.

We may presume that the reader is now pretty familiar with the different circumstances which must be considered in the construction of an eye-piece, and proceed to consider those which must be employed to erect the object.

This may be done by placing the lens which receives the light from the object-glass in such a manner, that a second image (inverted with respect to the first) may be formed beyond it, and this may be viewed by an eye-glass. Such a construction is represented in Fig. 23. But, besides many other defects, it tinges the object prodigiously with colour. The ray *o d* is dispersed at *d* into the red ray *d r*, and the violet *d v*, *v* being farther from the centre than *r*, the refracted ray *v v'* crosses *r r'* both by reason of spherical aberration, and its greater refrangibility.

But the common day telescope, invented by F. Rheita, has, in this respect, greatly the advantage of the one now described. The rays of compound light are dispersed. The violet ray falls without the red ray, but is accurately collected with it at the focus as we shall demonstrate by and by. Since they cross each other, the violet ray must fall within the red ray, and be less refracted than if it had fallen on the same point with the red ray. Had it fallen there it would have separated from it; but by a proper diminution of its refraction, it is kept parallel to it, or nearly so. And this is one excellence of this telescope; when constructed with three eye-glasses perfectly equal, the colour is sensibly diminished, and by using an eye-glass somewhat smaller, it may be removed entirely.—We say no more of it at present, because we shall find its construction included in another, which is still more perfect.

It is evident at first sight that this telescope may be improved, by substituting for the eye-glass, the Huyghenian double eye-glass, or field-glass and eye-glass represented in Fig. 19. and Fig. 20; and that the first of these may be improved and rendered achromatic. This will require the two glasses *e f* and *g h* to be increased from their present dimensions to the size of a field-glass, suited to the magnifying power of the telescope, supposing it an astronomical telescope. Thus we shall have a telescope of four eye-glasses. The three first will be of a considerable focal distance, and two of them will have a common focus at *b*. But this is considerably different from the eye-piece of four glasses which are now used, and are far better. We are indebted for them to Mr Dollond, who was a mathematician as well as an artist, and in the course of his research discovered resources which had not been thought of. He had not then discovered the achromatic object-glass, and was busy in improving the eye-glasses by diminishing their spherical aberration. His first thought was to make the Huyghenian addition at both the images of the day telescope. This suggested to him the following eye-piece of five glasses.

Fig. 24. represents this eye-piece, but there is not room for the object-glass at its proper distance. A pencil of rays coming from the upper point of the object is made to converge (by the object-glass) to G, where it would form a picture of that part of the object. But it is intercepted by the lens A *a*, and its axis is bent towards the axis of the telescope in the direction *a b*. At the same time, the rays which converged to G converge to *g*, and there is formed an inverted picture of the object at *g f*. The axis of the pencil is again refracted at *b*, crosses the axis of the telescope in H, is refracted again at *c*, at *d*, and at *e*, and at last crosses the axis in I. The rays of this pencil, diverging from *g*, are made less diverging, and proceed as if they came from *g*′, in the line B *g g*′. The lens *c* C causes them to converge to *g*′, in the line G″ C *g*′. The lens *d* D makes them converge still more to G″, and there they form an erect picture G″ F″; diverging from G″, they are rendered parallel by the refraction at *e*.

At H the rays are nearly parallel. Had the glass B *b* been a little farther from A, they would have been accurately so, and the object-glass, with the glasses A and B, would have formed an astronomical telescope with the Huyghenian eye-piece. The glasses C, D, and E, are intended merely for bending the rays back again till they again cross the axis in I. The glass C tends chiefly to diminish the great angle BH*b*; and then the two glasses D and E are another Huyghenian eye-piece.

The art in this construction lies in the proper adjustment of the glasses, so as to divide the whole bending of the pencil pretty equally among them, and to form the last image in the focus of the eye-glass, and at a proper distance from the other glass. Bringing B nearer to A would bend the pencil more to the axis. Placing C farther from B would do the same thing; but this would be accompanied with more aberration, because the rays would fall at a greater distance from the centres of the lenses. The greatest bending is made at the field-glass D; and we imagine that the telescope would be improved, and made more distinct at the edges of the field, by employing another glass of great focal distance between C and D.

There is an image formed at H of the object-glasses, and the whole light passes through a small circle in this place. It is usual to put a plate here pierced with a hole which has the diameter of this image. A second image of the object-glass is formed at I, and indeed wherever the pencils cross the axis. A lens placed at H makes no change in any of the angles, nor in the magnifying power, and affects only the place where the images are formed. And, on the other hand, a lens placed at *f*, or F″, where a real image is formed, makes no change in the places of the images, but affects the mutual inclination of the pencils. This affords a resource to the artist, by which he may combine properties which seem incompatible.

The aperture of A determines the visible field and all the other apertures.

We must avoid forming a real image, such as *fg*, or F″ G″, on or very near any glass. For we cannot see this image without seeing along with it every particle of dust and every scratch on the glass. We see them as making part of the object when the image is exactly on the glass, and we see them confusedly, and so as to confuse the object, when the image is near it. For when the image is on or very near any glass, the pencil of light occupies a very small part of its surface, and a particle of dust intercepts a great proportion of it.

It is plain that this construction will not do for the telescope of graduated instruments, because the micrometer cannot be applied to the second image *fg*, on account of its being a little distorted, as has been observed of the Huyghenian eye-piece.

Also the interposition of the glass C makes it difficult to correct the dispersion.

By proper reasoning from the correction in the Huyghenian eye-piece, we are led to the best construction of one with three glasses, which we shall now consider, taking it in a particular form, which shall make the discussion easy, and make us fully masters of the principles which lead to a better form. Therefore let PA (Fig. 25.) be the glass which first receives the light proceeding from the image formed by the object-glass, and let OP be the axis of the extreme pencil. This is refracted into PR, which is again refracted into R r by the next lens B r. Let b be the focus of parallel rays of the second lens. Draw PB r. We know that $\mathrm{A}b : b\mathrm{B} = \mathrm{PB} : \mathrm{B}r$, and that rays of one kind diverging from P will be collected at r. But if PR, PV be a red and a violet ray, the violet ray will be more refracted at V, and will cross the red ray in some intermediate point g of the line R r. If therefore the first image had been formed precisely on the lens PA, we should have a second image at fg free from all coloured fringes.

If the refractions at P and R are equal (as in the common day telescope), the dispersion at V must be equal to that at P, or the angle $v\mathrm{V}r = \mathrm{VPR}$. But we have ultimately $\mathrm{RPV} : \mathrm{R}r\mathrm{V} = \mathrm{BC} : \mathrm{AB}$, ($= \mathrm{B}b : \mathrm{A}b$ by the focal theorem). Therefore $g\mathrm{V}r : gr\mathrm{V}$ (or $gr : g\mathrm{V}$, or $\mathrm{C}f : f\mathrm{B}$) $= \mathrm{B}b : \mathrm{A}b$, and $\mathrm{AB} : \mathrm{A}b = \mathrm{R}r : \mathrm{R}g$.

This shows by the way the advantage of the common day telescope. In this $\mathrm{AB} = 2\,\mathrm{A}b$, and therefore f is the place of the last image which is free from coloured fringes. But this image will not be seen free from coloured fringes through the eye-glass C r, if f be its focus: For had $g\,r$, $g\,v$ been both red rays, they would have been parallel after refraction; but $g\,v$ being a violet ray, will be more refracted. It will not indeed be so much deflected from parallelism as the violet ray, which naturally accompanies the red ray to r, because it falls nearer the centre. By computation its dispersion is diminished about $\frac{1}{7}$th.

In order that $g\,v$ may be made parallel to $g\,r$ after refraction, the refraction at r must be such that the dispersion corresponding to it may be of a proper magnitude. How to determine this is the question. Let the dispersion at g be to the dispersion produced by the refraction at r (which is required for producing the intended magnifying power) as 1 to 9. Make $9 : 1 = ff' : f'C, = fC : CD$, and draw the perpendicular $D\,r'$ meeting the refracted ray $r\,r'$ in r'. Then we know by the common focal theorem, that if f' be the focus of the lens $C\,r$, red rays diverging from g will be united in r'. But the violet ray $g\,v$ will be refracted into $v\,v'$ parallel to $r\,r'$. For the angle $v\,r'\,r : v\,g\,r =$ (ultimately) $fC : CD, = 9 : 1$. Therefore the angle $v\,r'\,r$ is equal to the dispersion produced at r, and therefore equal to $r'\,v\,v'$, and $v\,v'$ is parallel to $r\,r'$.

But by this we have destroyed the distinct vision of the image formed at fg, because it is no longer at the focus of the eye-glass. But distinct vision will be restored by pushing the glasses nearer to the object-glass. This makes the rays of each particular pencil more divergent after refraction through A, but scarcely makes any change in the directions of the pencils themselves. Thus the image comes to the focus f', and makes no sensible change in the dispersions.

In the common day telescope, the first image is formed in the anterior focus of the first eye-glass, and the second image is at the anterior focus of the last eye-glass. If we change this last for one of half the focal distance, and push in the eye-piece till the image formed by the object-glass is half way between the first eye-glass and its focus, the last image will be formed at the focus of the new eye-glass, and the eye-piece will be achromatic. This is easily seen by making the usual computations by the focal theorem. But the visible field is diminished, because we cannot give the same aperture as before to the new eye-glass; but we can substitute for it two eye-glasses like the former, placed close together. This will have the same focal distance with the new one, and will allow the same aperture that we had before.

On these principles may be demonstrated the correction of colour in eye-pieces with three glasses of the following construction:

Let the glasses A and B be placed so that the posterior focus of the first nearly coincides with the anterior focus of the second, or rather so that the anterior focus of B may be at the place where the image of the object-glass is formed, by which situation the aperture necessary for transmitting the whole light will be the smallest possible. Place the third C at a distance from the second, which exceeds the sum of their focal distances by a space which is a third proportional to the distance of the first and second, and the focal distance of the second. The distance of the first eye-glass from the object-glass must be equal to the product of the focal distance of the first and second divided by their sum.

Let O o, A a, B b, C c, the focal distances of the glasses, be O, a, b, c. Then make $AB = a + b$ nearly; $BC = b + c + \frac{b^2}{b+c}$; $OA = \frac{bc}{b+c}$. The amplification or magnifying power will be $= \frac{ob}{ac}$; the equivalent eye-glass $= \frac{ac}{b}$; and the field of vision $= 3438' \times \frac{\text{Aperture of A}}{\text{foc. dist. ob. gl.}}$

These eye-pieces will admit the use of a micrometer at the place of the first image, because it has no distortion.

Mr Dollond was anxious to combine this achromatism in the eye-pieces with the advantages which he had found in the eye-pieces with five glasses. This eye-piece of three glasses necessarily has a very great refraction at the glass B, where the pencil which has come from the other side of the axis must be rendered again convergent, or at least parallel to it. This occasions considerable aberrations. This may be avoided by giving part of this refraction to a glass put between the first and second, in the same way as he has done by the glass B put between A and C in his five-glass eye-piece. But this deranges the whole process. His ingenuity, however, surmounted this difficulty, and he made eye-pieces of four glasses, which seem as perfect as can be desired. He has not published his ingenious investigation; and we observe the London artists work very much at random, probably copying the proportions of some of his best glasses, without understanding the principle, and therefore frequently mistaking. We see many eye-pieces which are far from being achromatic. We imagine, therefore, that it will be an acceptable thing to the artists to have precise instructions how to proceed, nothing of this kind having appeared in our language, and the investigations of Euler, D'Alembert, and even Boscovich, being so abstruse as to be inaccessible to all but experienced analysts. We hope to render it extremely simple.

It is evident, that if we make the rays of different colours unite on the surface of the last eye-glass but one, commonly called the *field-glass*, the thing will be done, because the dispersion from this point of union will then unite with the dispersion produced by this glass alone; and this increased dispersion may be corrected by the last eye-glass in the way already shown.

Therefore let A, B (Fig. 26.) be the stations which we have fixed on for the first and second eye-glasses, in order to give a proper portion of the whole refraction to the second glass. Let b be the anterior focus of B. Draw PBr through the centre of B. Make $\mathrm{A}b : b\mathrm{B} = \mathrm{AB} : \mathrm{BK}$. Draw the perpendicular Kr, meeting the refracted ray in r. We know by the focal theorem, that red rays diverging from P will converge to r; but the violet ray PV, being more refracted, will cross Rr in some point g. Drawing the perpendicular fg, we get f for the proper place of the field-glass. Let the refracted ray Rr, produced backwards, meet the ray OP coming from the centre of the object-glass in O. Let the angle of dispersion RPV be called p, and the angle of dispersion at V, that is, rVv, be v, and the angle VrR be r.

It is evident that OR : OP $= p : v$, because the dispersions are proportional to the sines of the refractions, which, in this case, are very nearly as the refractions themselves.

Let $\frac{\mathrm{OP}}{\mathrm{OR}} \left(\text{or } \frac{op}{p\mathrm{B}} \text{ or } \frac{p\mathrm{B}}{b\,\mathrm{B}}\right)$ be made $= m$. Then $v = mp$; also $p : r = \mathrm{BK} : \mathrm{AB}, = b\,\mathrm{B} : \mathrm{A}\,b$, and $r = p.\ \frac{\mathrm{A}b}{\mathrm{B}b}$, or, making $\frac{\mathrm{A}b}{\mathrm{B}b} = n$, $r = np$; therefore $v : r = m : n, = \frac{p\mathrm{B}}{b\,\mathrm{B}} : \frac{\mathrm{A}b}{b\,\mathrm{B}}, = p\,\mathrm{B} : \mathrm{A}\,b$.

The angle $\mathrm{R}\,g\,\mathrm{V} = g\,\mathrm{V}\,r + g\,r\,\mathrm{V} = p.\,\overline{m+n}$; and $\mathrm{R}\,g\,\mathrm{V} : \mathrm{R}\,r\,v = \mathrm{R}\,r : \mathrm{R}\,g$, or $m + n : n = \mathrm{R}\,r : \mathrm{R}\,g$, and $\mathrm{R}\,g = \mathrm{R}\,r\,\frac{n}{m+n}$. But R r is ultimately $= \mathrm{BK} = \mathrm{AB}\,\frac{b\,\mathrm{B}}{\mathrm{A}b} = \frac{\mathrm{AB}}{n}$. Therefore $\mathrm{R}\,g = \frac{\mathrm{AB}}{n} \times \frac{n}{m+n} = \frac{\mathrm{AB}}{m+n}$, and $\mathrm{B}f = \frac{\mathrm{AB}}{m+n}$.

This value of Bf is evidently $= b\,\mathrm{B} \times \frac{\mathrm{AB}}{p\,\mathrm{B} + \mathrm{A}\,b}$. Now b B being a constant quantity while the glass B is the same, the place of union varies with $\frac{\mathrm{AB}}{p\,\mathrm{B} + \mathrm{A}\,b}$. If we remove B a little farther from A, we increase AB, and pB, and A b, each by the same quantity. This evidently diminishes Bf. On the other hand, bringing B nearer to A increases Bf. If we keep the distance between the glasses the same, but increase the focal distance, b B, we augment Bf, because this change augments the numerator and diminishes the denominator of the fraction $\frac{b\,\mathrm{B} \times \mathrm{A\,B}}{p\,\mathrm{B} + \mathrm{A}\,b}$.

In this manner we can unite the colours at what distance we please, and consequently can unite them in the place of the intended field-glass, from which they will diverge with an increased dispersion, viz. with the dispersion competent to the refraction produced there, and the dispersion $p \times \overline{m+n}$ conjoined.

It only remains to determine the proper focal distances of the field-glass and eye-glass, and the place of the eye-glass, so that this dispersion may be finally corrected.

This is an indeterminate problem, admitting of an infinity of solutions. We shall limit it by an equal division of the two remaining refractions, which are necessary in order to produce the intended magnifying power. This construction has the advantage of diminishing the aberration. Thus we know the two refractions, and the dispersion competent to each; it being nearly $_{27}$th of the refraction. Call this q. The whole dispersion at the field-glass consists of q, and of the angle K g V of Fig. 26, which we also know to be $= p \times \overline{m+n}$. Call their sum s.

Let Fig. 27. represent this addition to the eye-piece. C g is the field-glass coming in the place of $f g$ of Fig. 26. and R g w is the red ray coming from the glass BR. Draw g s parallel to the intended emergent pencil from the eye-glass; that is, making the angle C s g with the axis correspond to the intended magnifying power. Bisect this angle by the line g K. Make $s g : g q = s : q$, and draw q K, cutting C g in t. Draw t δ D, cutting g k in δ, and the axis in D. Draw δ d and D r perpendicular to the axis. Then a lens placed in D, having the focal distance D d, will destroy the dispersion at the lens g e, which refracts the ray g w into g r.

Let g v be the violet ray, making the angle $v g r = s$. It is plain, by the common optical theorem, that g r will be refracted into r r' parallel to δ D. Draw g D r' meeting r r', and join v r'. By the focal theorem two red rays g r, g v, will be united in r'. But the violet ray g v will be more refracted, and will take the path v v', making the angle of dispersion $r' v v' = q$, very nearly, because the dispersion at v does not sensibly differ from that at r. Now, in the small angles of refraction which obtain in optical instruments, the angles $r\,r'\,v$, $r\,g\,v$ are very nearly as $g\,r$ and $r\,r'$, or as g D and D r', or as CD and DT; which, by the focal theorem, are as C d and d D; that is, D $d : d\,c = r\,g\,v : r\,r'\,v$. But D $d : d$ C $=$ D $\delta : \delta\,t$, $= s\,g : g\,q$, $= s : q$. But $r\,g\,v = s$; therefore $r\,r'\,v = q$, $= r'\,v\,v'$, and $v\,v'$ is parallel to $r\,r'$, and the whole dispersion at g is corrected by the lens D r. The focal distance C c of C g is had by drawing C x parallel to K g, meeting R g in x, and drawing $x\,c$ perpendicular to the axis.

It is easy to see that this (not inelegant) construction is not limited to the equality of the refractions $w\,g\,r$, K $r\,r'$. In whatever proportion the whole refraction $w\,g\,s$ is divided, we always can tell the proportion of the dispersions which the two refractions occasion at g and r, and can therefore find the values of s and q. Indeed this solution includes the problem in p. 504; but it had not occurred to us till the present occasion. Our readers will not be displeased with this variety of resource.

The intelligent reader will see, that in this solution some quantities and ratios are assumed as equal which are not strictly so, in the same manner as in all the elementary optical theorems. The parallelism, however, of $v\,v'$ and $r\,r'$ may be made accurate, by pushing the lens D r nearer to C g, or retiring it from it. We may also, by pushing it still nearer, induce a small divergency of the violet ray, so as to produce accurate vision in the eye, and may thus make the vision through a telescope more perfect than with the naked eye, where dispersion is by no means avoided. It would therefore be an improvement to have the eye-glass in a sliding tube for adjustment. Bring the telescope to distinct vision; and if any colour be visible about the edges of the field, shift the eye-glass till this colour is removed. The vision may now become indistinct: but this is corrected by shifting the place of the whole eye-piece.

We have examined trigonometrically the progress of a red and a violet ray through many eye-pieces of Dollond's and Ramsden's best telescopes; and we have found in all of them that the colours are united on or very near the field-glass; so that we presume that a theory somewhat analogous to ours has directed the ingenious inventors. We meet with many made by other artists, and even some of theirs, where a considerable degree of colour remains, sometimes in the natural order, and often in the contrary order. This must happen in the hands of mere imitators, ignorant of principle. We presume that we have now made this principle sufficiently plain.

Fig. 28. represents the eye-piece of a very fine spy-glass by Mr Ramsden; the focal length of its object-glass is 8½ inches, with 1$\frac{1}{10}$th of aperture, 2° 05′ of visible field, and 15,4 magnifying power. The distances and focal lengths are of their proper dimensions, but the apertures are ½ larger, that the progress of a lateral pencil might be more distinctly drawn. The dimensions are as follow:

Foc. lengths $Aa=0,0775$ $Bb=1,025$ $Cc=1,01$ $Dd=0, 79$
Distances $AB=1,18$ $BC=1,83$ $CD=1,105$.

It is perfectly achromatic, and the colours are united, not precisely, at the lens C *g*, but about $\frac{1}{20}$th of an inch nearer the eye-glass.

It is obvious that this combination of glasses may be used as a microscope; for if, instead of the image formed by the object-glass at FG, we substitute a small object, illuminated from behind, as in compound microscopes; and if we draw the eye-piece a very small way from this object, the pencils of parallel rays emergent from the eye-glass D will become convergent to very distant points, and will there form an inverted and enlarged picture of the object, which may be viewed by a Huyghenian eye-piece; and we may thus get high magnifying powers without using very deep glasses. We tried the eye-piece of which we have given the dimensions in this way, and found that it might be made to magnify 180 times with very great distinctness. When used as the magnifier of a solar microscope, it infinitely surpasses every thing we have ever seen. The picture formed by a solar microscope is generally so indistinct, that it is fit only for amusing ladies; but with this magnifier it seemed perfectly sharp. We therefore recommend this to the artists as a valuable article of their trade.

The only thing which remains to be considered in the theory of refracting telescopes is the forms of the different lenses. Hitherto we have had no occasion to consider any thing but their focal distances; but their aberrations depend greatly on the adjustments of their forms to their situations. When the conjugate focuses of a lens are determined by the service which it is to perform, there is a certain form or proportion between the curvatures of their anterior and posterior surfaces, which will make their aberrations the smallest possible.

It is evident that this proportion is to be obtained by making the fluxion of the quantity within the parenthesis in the formula of page 442, line 12, equal to nothing. When this is done, we obtain this formula for a, the radius of curvature for the anterior surface of a lens. $\frac{1}{a} = \frac{2m^2+m}{2m+4} + \frac{4m+4}{2(m+4)r}$, where m is the ratio of the sine of incidence to the sine of refraction, and r is the distance of the focus of incident rays, positive or negative, according as they converge or diverge, all measured on a scale of which the unit is n, $=$ half of the radius of the equivalent isosceles lens.

It will be sufficiently exact for our purpose to suppose $m = \frac{3}{2}$, though it is more nearly $\frac{31}{20}$. In this case $\frac{1}{a} = \frac{b}{7} + \frac{10}{7r}, = \frac{42r+70}{49r}$. Therefore $a = \frac{49r}{42r+70}$. And $\frac{1}{b} = \frac{1}{a} - 1, = \frac{1-a}{a}$

As an example, let it be required to give the radii of curvature in inches for the eye-glass *b e* of page 468, which we shall suppose of 1½ inches focal distance, and that $e\,c\ (=r)$ is 3¾th inches.

The radius of curvature for the equivalent isosceles lens is 1,5, and its half is 0,75. Therefore $r = \frac{3\frac{3}{4}}{0,75}, = 5$; and our formula is $a = \frac{49 \times 5}{42 \times 3 + 70}, = \frac{245}{280}, = 0,875$; and $\frac{1}{b} = \frac{1-a}{a}, = \frac{0,125}{0,875}$, and $b = \frac{0,875}{0,125}, = 7$.

These values are parts of a scale, of which the unit is 0,75 inches. Therefore

$$a\text{, in inches,} = 0,875 \times 0,75, = 0,65525$$
$$b\text{, in inches,} = 7 \times 0,75, \quad = 5,25.$$

And here we must observe that the posterior surface is concave: for b is a positive quantity, because $1-a$ is a positive quantity as well as a; therefore the centre of sphericity of both surfaces lies beyond the lens.

And this determination is not very different from the usual practice, which commonly makes this lens a plane convex with its flat side next the eye: and there will not be much difference in the performance of these two lenses; for in all cases of maxima and minima, even a pretty considerable change of the best dimensions does not make a sensible change in the result.

The same consideration leads to a rule which is very simple and sufficiently exact for ordinary situations. This is to make the curvatures such, that the incident and emergent pencils may be nearly equally inclined to the surfaces of the lens. Thus in the eye-piece with five glasses, A and B should be most convex on their anterior sides; C should be most convex on the posterior side; D should be nearly isosceles; and E nearly plano-convex.

But this is not so easy a matter as appears at first sight. The lenses of an eye-piece have not only to bend the several pencils of light to and from the axis of the telescope; they have also to form images on the axis of these pencils. These offices frequently require opposite forms. Thus the glass A of Fig. 28. should be most convex on the side next the object, that it may produce little distortion of the pencils. But it should be most convex next the eye, that it may produce distinct vision of the image FG, which is very near it. This image should have its concavity turned towards A, whereas it is towards the object-glass. We must therefore endeavour to make the vertical image *fg* flatter, or even convex. This requires a glass very flat before, and convex behind. For similar reasons the object-glass of a microscope and the simple eye-glass of an astronomical telescope should be formed the same way.

This is a subject of most difficult discussion, and requires a theory which few of our readers would relish; nor does our limits afford room for it. The artists are obliged to grope their way. The proper method of experiment would be, to make eye-piecesof large dimensions, with extravagant apertures to increase the aberrations, and to provide for each station A, B, C, and D, a number of lenses of the same focal distance, but of different forms: and we would advise making the trial in the way of a solar microscope, and to have two eye-pieces on trial at once. Their pictures can be formed on the same screen, and accurately compared; whereas it is difficult to keep in remembrance the performance of one eye-piece, and compare it with another.

We have now treated the theory of refracting telescopes with considerable minuteness, and have perhaps exceeded the limits which some readers may think reasonable. But we have long regretted that there is not any theory on this subject from which a curious person can learn the improvements which have been made since the time of Dr Smith, or an artist learn how to proceed with intelligence in his profession. If we have accomplished either of these ends, we trust that the public will receive our labours with satisfaction.

We cannot add any thing to what Dr Smith has delivered on the theory of reflecting telescopes. There appears to be the same possibility of correcting the aberration of the great speculum by the contrary aberration of a convex small speculum, that we have practised in the compound object-glass of an achromatic refracting telescope. But this cannot be, unless we make the radius of the convex speculum exceedingly large, which destroys the magnifying power and the brightness. This therefore must be given up. Indeed their performance, when well executed, does already surpass all imagination. Dr Herschel has found great advantages in what he calls the *front view*, not using a plane mirror to throw the pencils to one side. But this cannot be practised in any but telescopes so large, that the loss of light, occassioned by the interposition of the observer's head, may be disregarded

NOTHING remains but to describe the mechanism of some of the most convenient forms.

To describe all the varieties of shape and accommodation which may be given to a telescope, would be a task as trifling as prolix. The artists of London and of Paris have racked their inventions to please every fancy, and to suit every purpose. We shall content ourselves with a few general maxims, deduced from the scientific consideration of a telescope, as an instrument by which the visual angle subtended by a distinct object is greatly magnified.

The chief consideration is to have a steady view of the distant object. This is unattainable, unless the axis of the instrument be kept constantly directed to the same point of it: for when the telescope is gently shifted from its position, the object *seems to move* in the same or in the opposite direction, according as the telescope inverts the object or shows it erect. This is owing to the magnifying power, because the apparent angular motion is greater than what we naturally connect with the motion of the telescope. This does not happen when we look through a tube without glasses.

All shaking of the instrument therefore makes the object dance before the eye; and this is disagreeable, and hinders us from seeing it distinctly. But a tremulous motion, however small, is infinitely more prejudicial to the performance of a telescope, by making the object quiver before us. A person walking in the room prevents us from seeing distinctly; nay, the very pulsation in the body of the observer agitates the floor enough to produce this effect, when the telescope has a great magnifying power: For the visible motion of the object is then an imperceptible tremor, like that of an harpsichord wire, which produces an effect precisely similar to optical indistinctness; and every point of the object is diffused over the whole space of the angular tremor, and appears coexistent in every part of this space, just as a harpsichord wire does while it is sounding. The more rapid this motion is, the indistinctness is the more complete. Therefore the more firm and elastic and well bound together the frame-work and apertures of our telescope is, the more hurtful will this consequence be. A mounting of lead, were it practicable, would be preferable to wood, iron, or brass. This is one great cause of the indistinctness of the very finest reflecting telescopes of the usual constructions, and can never be totally removed. In the Gregorian form, it is hardly possible to damp the elastic tremor of the small speculum, carried by an arm supported at one end only, even though the tube were motionless. We were witnesses of a great improvement made on a four-feet reflecting telescope, by supporting the small speculum by a strong plate of lead placed across the tube, and led by an adjusting screw at each end. But even the great mirror may vibrate enough to produce indistinctness. Refracting telescopes are free from this inconveniency, because a small angular motion of the object-glass round one of its own diameters has no sensible effect on the image in its focus. They are affected only by an angular motion of the axis of the telescope or of the eye-glasses.

This single consideration gives us great help towards judging of the merits of any particular apparatus. We should study it in this particular, and see whether its form makes the tube readily susceptible of such tremulous motions. If it does, the firmer it is, and the more elastic it is, the worse. All forms therefore where the tube is supported only near the middle, or where the whole immediately or remotely depend on one narrow joint, are defective.

Reasoning in this way, we say with confidence, that of all the forms of a telescope apparatus, the old-fashioned simple stand represented in Fig. 29. is by far the best, and that others are superior according as the disposition of the points of support of the tube approaches to this. Let the pivots A, B, be fixed in the lintel and sole of a window. Let the four braces terminate very near to these pivots. Let the telescope lie on the pin F*f*, resting on the shoulder round the eye-piece, while the far end of it rests on one of the pins 1, 2, 3, &c.; and let the distance of these pins from F very little exceed the length of the telescope. The trembling of the axis, even when considerable, cannot affect the position of the tube, because the braces terminate almost at the pivots. The tremor of the brace CD does as little harm, because it is nearly perpendicular to the tube. And if the object-glass were close at the upper supporting pin, and the focus at the lower pin F, even the bending and trembling of the tube will have no effect on its optical axis. The instrument is only subject to horizontal tremors. These may be almost annihilated by having a slender rod coming from a hook's joint in the side of the window, and passing through such another joint close by the pin F. We have seen an instrument of this form, having AB parallel to the earth's axis. The whole apparatus did not cost 50 shillings, and we find it not in the least sensible manner affected by a storm of wind. It was by observations with this instrument that the tables of the motions of the Georgium Sidus, published in the Edinburgh Transactions, were constructed, and they are as accurate as any that have yet appeared. This is an excellent equatorial:

But this apparatus is not portable, and it is sadly deficient in elegance. The following is the best method we have seen of combining these circumstances with the indispensable requisities of a good telescope.

The pillar VX (Fig. 30.) rises from a firm stand, and has a horizontal motion round a cone which completely fills it. This motion is regulated by a rack-work in the box at V. The screw of this rack-work is turned by means of the handle P, of a convenient length, and the screw may be disengaged by the click or detent V, when we would turn the instrument a great way at once. The telescope has a vertical motion round the joint Q placed near the middle of the tube. The lower end of the tube is supported by the stay OT. This consists of a tube RT, fastened to the pillar by a joint T, which allows the stay to move in a vertical plane. Within this tube slides another, with a stiff motion. This tube is connected with the telescope by another joint O, also admitting motion in a vertical plane. The side M of this inner tube is formed into a rack, in which works a pinion fixed to the top of the tube RT, and turned by the flat finger-piece R. The reader will readily see the advantages and the remaining defects of this apparatus. It is very portable, because the telescope is easily disengaged from it, and the legs and stay fold up. If the joint Q were immediately under A, it would be much freer from all tremor in the vertical plane. But nothing can hinder other tremors arising from the long pillar and the three springy legs. These communicate all external agitations with great vigour. The instrument should be set on a stone pedestal, or, what is better, a cask filled with wet sand. This pedestal, which necessity perhaps suggested to our scientific navigators, is the best that can be imagined.

Fig. 31. is the stand usually given to reflecting telescopes. The vertical tube FBG is fastened to the tube by finger-screws, which pass through the slits at F and G. This arch turns round a joint in the head of the divided pillar, and has its edge cut into an oblique rack, which is acted on by the horizontal screw, furnished with the finger-piece A. This screw turns in a horizontal square frame. This frame turns round a horizontal joint in the off side, which cannot be seen in this view. In the side of this frame next the eye there is a finger-screw *a*, which passes through the frame, and presses on the round horizontal plate D. By screwing down this finger-screw, the frame is brought up, and presses the horizontal screw to the rack. Thus the elevation of the telescope is fixed, and may be nicely changed by the finger applied to A and turning this screw. The horizontal round plate D moves stiffly round on another plate of nearly equal diameter. This under plate has a deep conical hollow socket, which is nicely fitted by grinding to a solid cone formed on the top of the great upright pillar, and they may be firmly fixed in any position by the finger-screw E. To the under plate is fastened a box *c*, containing a horizontal screw C, which always works in a rack cut in the edge of the upper plate, and cannot be disengaged from it. When a great vertical or horizontal motion is wanted, the screws *a* and E are slacked, and by tightening them the telescope may be fixed in any position, and then any small movements may be given it by the finger plates A and C.

This stand is very subject to brisk tremor, either from external agitation of the pedestal, or from the immediate action of the wind; and we have seldom seen distinctly through telescopes mounted in this manner, till one end of the tube was pressed against something that was very steady and unelastic. It is quite astonishing what a change this produces. We took a very fine telescope made by James Short, and laid the tube on a great lump of soft clay, pressing it firmly down into it. Several persons, ignorant of our purpose, looked through it, and read a table of logarithms at the distance of 310 yards. We then put the telescope on its stand, and pointed it to the same object; none of the company could read at a greater distance than 235 yards, although they could perceive no tremor. They thought the vision as sharp as before; but the incontrovertible proof of the contrary was, that they could not read at such a distance.

If the round plates were of much greater dimensions; and if the lower one, instead of being fixed to the pillar, were supported on four stout pillars standing on another plate; and if the vertical arch had a horizontal axis turning on two upright frames firmly fixed to the upper plate—the instrument would be much freer from tremor. Such stands were made formerly; but being much more bulky and inconvenient for package, they have gone into disuse.

The high magnifying powers of Dr Herschel's telescopes made all the usual apparatus for their support extremely imperfect. But his judgment, and his ingenuity and fertility in resource, are as eminent as his philosophical ardour. He has contrived for his reflecting telescopes stands which have every property that can be desired. The tubes are all supported at the two ends. The motions, both vertical and horizontal, are contrived with the utmost simplicity and firmness. We cannot more properly conclude this article, than with a description of his 40-feet telescope, the noblest monument of philosophical zeal and of princely munificence that the world can boast of.

Fig. 32. represents a view of this instrument in a meridional situation, as it appears when seen from a convenient distance by a person placed to the south-west of it.

The foundation in the ground consists of two concentric circular brick walls, the outermost of which is 42 feet in diameter, and the inside one 21 feet. They are two feet six inches deep under ground; two feet three inches broad at the bottom, and one foot two inches at the top; and are capped with paving stones about three inches thick, and twelve and three quarters broad. The bottom frame of the whole apparatus rests upon these two walls by twenty concentric rollers I, I, I, and is moveable upon a pivot, which gives a horizontal motion to the whole apparatus, as well as to the telescope.

The tube of the telescope A, though very simple in its form, which is cylindrical, was attended with great difficulties in the construction. This is not to be wondered at, when its size, and the materials of which it is made, are considered. Its length is 39 feet four inches; it measures four feet ten inches in diameter; and every part of it is of iron. Upon a moderate computation, the weight of a wooden tube must have exceeded an iron one at least 3000 pounds; and its durability would have been far inferior to that of iron. It is made of rolled or sheet iron, which has been joined together without rivets, by a kind of seaming well known to those who make iron-funnels for stoves.

Very great mechanical skill is used in the contrivance of the apparatus by which the telescope is supported and directed. In order to command every altitude, the point of support is moveable; and its motion is effected by mechanism, so that the telescope may be moved from its most backward point of support to the most forward, and, by means of the pulleys GG suspended from the great beam H, be set to any altitude, up to the very zenith. The tube is also made to rest with the point of support in a pivot, which permits it to be turned sidewise.

The concave face of the great mirror is 48 inches of polished surface in diameter. The thickness, which is equal in every part of it, remains now about three inches and a half; and its weight, when it came from the cast, was 2118 pounds, of which it must have lost a small quantity in polishing. To put this speculum into the tube, it is suspended vertically by a crane in the laboratory, and placed on a small narrow carriage, which is drawn out, rolling upon planks, till it comes near the back of the tube; here it is again suspended and placed in the tube by a peculiar apparatus.

The method of observing by this telescope is by what Dr Herschel calls the *front view;* the observer being placed in a seat C, suspended at the end of it, with his back towards the object he views. There is no small speculum, but the magnifiers are applied immediately to the first focal image.

From the opening of the telescope, near the place of the eye-glass, a speaking pipe runs down to the bottom of the tube, where it goes into a turning joint; and after several other inflections, it at length divides into two branches, one going into the observatory D, and the other into the workroom E. By means of the speaking pipe the communications of the observer are conveyed to the assistant in the observatory, and the workman is directed to perform the required motions.

In the observatory is placed a valuable sidereal timepiece, made by Mr Shelton. Close to it, and of the same height, is a polar distance-piece, which has a dial-plate of the same dimensions with the time-piece: this piece may be made to show polar distance, zenith distance, declination or altitude, by setting it differently. The time and polar distance pieces are placed so that the assistant sits before them at a table, with the speaking-pipe rising between them; and in this manner observations may be written down very conveniently.

This noble instrument, with proper eye-glasses, magnifies above 6000 times, and is the largest that has ever been made. Such of our readers as wish for a fuller account of the machinery attached to it, viz. the stairs, ladders, and platform B, may have recourse to the second part of the Transactions of the Royal Society for 1795; in which, by means of 18 plates and 63 pages of letter-press, an ample detail is given of every circumstance relating to joiner's work, carpenter's work, and smith's work, which attended the formation and erection of this telescope. It was completed on August the 28th, 1789, and on the same day was the sixth satellite of Saturn discovered.

PNEUMATICS.

1. This term is restricted, in the present habits of our language, to that part of natural philosophy which treats of the mechanical properties of elastic fluids. The word, in its original meaning, expresses a quality of air, or, more properly, of breath.

2. We have extended (on the authority of present custom) the term Pneumatics to the study of the mechanical properties of all elastic or sensibly compressible fluids, that is, of fluids whose elasticity and compressibility become an interesting object of our attention; as the term Hydrostatics is applied to the study of the mechanical properties of such bodies as interest us by their fluidity or liquidity only, or whose elasticity and compressibility are not familiar or interesting, though not less real or general than in the case of air and all vapours.

3. Of all the sensibly compressible fluids air is the most familiar, was the first studied, and the most minutely examined. It has therefore been generally taken as the example of their mechanical properties, while those mechanical properties which are peculiar to any of them, and therefore characteristic, have usually been treated as an appendix to the general science of pneumatics. No objection oc-

curs to us against this method, which will therefore be adopted in treating this article.

4. But although the mechanical properties are the porper subjects of our consideration, it will be impossible to avoid considering occasionally properties which are more of a chemical nature; because they occasion such modifications of the mechanical properties as would frequently be unintelligible without considering them in conjunction with the other; and, on the other hand, the mechanical properties produce such modifications of the properties merely chemical, and of very interesting phenomena consequent on them, that these would often pass unexplained unless we give an account of them in this place.

5. By *mechanical properties* we would be understood to mean such as produce, or are connected with, sensible changes of motion, and which indicate the presence and agency of moving or mechanical powers. They are therefore the subject of mathematical discussion; admitting of measure, number, and direction, notions purely mathematical.

We shall therefore begin with the consideration of air.

6. It is by no means an idle question, "*What is this* air of which so much is said and written?" We see nothing, we feel nothing. We find ourselves at liberty to move about in any direction without any let or hinderance. Whence, then, the assertion, that we are surrounded with a *matter* called air? A few very simple observations and experiments will show us that this assertion is well-founded.

7. We are accustomed to say, that a vessel is empty when we have poured out of it the water which it contained. Take a cylindrical glass jar (Plate IX. Fig. 1.), having a small hole in its bottom; and having stopped this hole, fill the jar with water, and then pour out the water, leaving the glass empty, in the common acceptation of the word. Now, throw a bit of cork, or any light body, on the surface of

water in a cistern: cover this with the glass jar held in the hand with its bottom upwards, and move it downwards, keeping it all the while in an upright position. The cork will continue to float on the surface of the water in the inside of the glass, and will most distinctly show whereabouts that surface is. It will thus be seen, that the water within the glass has its surface considerably lower than that of the surrounding water; and however deep we immerge the glass, we shall find that the water will never rise in the inside of it so as to fill it. If plunged to the depth of 32 feet, the water will only half fill it; and yet the acknowledged laws of hydrostatics tell us, that the water would fill the glass if there were nothing to hinder it. There is therefore something already within the glass which prevents the water from getting into it; manifesting in this manner the most distinctive property of matter, viz. the hindering other matter from occupying the same place at the same time.

8. While things are in this condition, pull the stopper out of the hole in the bottom of the jar, and the water will instantly rise in the inside of the jar, and stand at an equal height within and without. This is justly ascribed to the escape through the hole of the *matter* which formerly obstructed the entry of the water: for if the hand be held before the hole, a puff will be distinctly felt, or a feather held there will be blown aside; indicating in this manner, that what prevented the entry of the water, and now escapes, possesses another characteristic property of matter, *impulsive force*. The materiality is concluded from this appearance in the same manner that the materiality of water is concluded from the impulse of a jet from a pipe. We also see the mobility of the formerly pent up, and now liberated, substance, in consequence of external pressure, viz. the pressure of the surrounding water.

9. Also, if we take a smooth cylindrical tube, shut at one end, and fit a plug or cork to its open end, so as to

slide along it, but so tightly as to prevent all passage by its sides; and if the plug be well soaked in grease, we shall find that no force whatever can push it to the bottom of the tube. There is therefore *something* within the tube preventing, by its impenetrability, the entry of the plug, and therefore possessing this characteristic of matter.

10. In like manner, if, after having opened a pair of common bellows, we shut up the nozzle and valve hole, and try to bring the boards together, we find it impossible. There is something included which prevents this, in the same manner as if the bellows were filled with wool; but on opening the nozzle we can easily shut them, viz. by expelling this something; and if the compression is forcible, the something will issue with considerable force, and very sensibly impel any thing in its way.

11. It is not accurate to say, that we move about without *any* obstruction; for we find, that if we endeavour to move a large fan with rapidity, a very sensible hinderance is perceived, and that a very sensible force must be exerted; and a sensible wind is produced, which will agitate the neighbouring bodies. It is therefore justly concluded, that the motion is possible only in consequence of having driven this obstructing substance out of the way; and that this impenetrable, resisting, moveable, impelling substance, *is matter*. We perceive the perseverance of this matter in its state of rest when we wave a fan, in the same manner that we perceive the *inertia* of water when we move a paddle through it. The effects of wind in impelling our ships and mills, in tearing up trees, and overturning buildings, are equal indications of its perseverance in a state of motion.

To this matter, when at rest, we give the name AIR; and when it is in motion we call it WIND.

Air, therefore, is a material fluid: a fluid, because its parts are easily moved, and yield to the smallest inequality of pressure.

Air possesses some others of the very general, though not essential, properties of matter. It is heavy. This appears from the following facts:

1. It always accompanies this globe in its orbit round the sun, surrounding it to a certain distance, under the name of the ATMOSPHERE, which indicates the being connected with the earth by its general force of gravity. It is chiefly in consequence of this that it is continually moving round the earth from east to west; forming what is called the trade-wind, to be more particularly considered afterwards. All that is to be observed on this subject at present is, that in consequence of the disturbing force of the sun and moon, there is an accumulation of the air of the atmosphere, in the same manner as of the waters of the ocean, in those parts of the globe which have the moon near their zenith or nadir: and as this happens successively, going from the east to the west (by the rotation of the earth round its axis in the opposite direction), the accumulated air must gradually flow along to form the elevation. This is chiefly to be observed in the torrid zone; and the generality and regularity of this motion are greatly disturbed by the changes which are continually taking place in different parts of the atmosphere from causes which are not mechanical.

2. It is in like manner owing to the gravity of the air that it supports the clouds and vapours which we see constantly floating in it. We have even seen bodies of no inconsiderable weight float, and even rise, in the air. Soap bubbles, and balloons filled with inflammable gas, rise and float in the same manner as a cork rises in water. This phenomenon proves the weight of the air in the same manner that the swimming of a piece of wood indicates the weight of the water which supports it.

3. But we are not left to these refined observations for the proof of the air's gravity. We may observe familiar phenomena, which would be immediate consequences of

the supposition that air is a heavy fluid, and, like other heavy fluids, presses on the outsides of all bodies immersed in or surrounded by it. Thus, for instance, if we shut the nozzle and valve hole of a pair of bellows after having squeezed the air out of them, we shall find that a very great force, even some hundred pounds, is necessary for separating the boards. They are kept together by the pressure of the heavy air which surrounds them in the same manner as if they were immersed in water. In like manner, if we stop the end of a syringe after its piston has been pressed down to the bottom, and then attempt to draw up the piston, we shall find a considerable force necessary, viz. about 15 or 16 pounds for every square inch of the section of the syringe. Exerting this force, we can draw up the piston to the top, and we can hold it there; but the moment we cease acting, the piston rushes down and strikes the bottom. It is called a suction, as we feel something as it were drawing in the piston; but it is really the weight of the incumbent air pressing it in. And this obtains in every position of the syringe; because the air is a fluid, and presses in every direction. Nay, it presses on the syringe as well as on the piston; and if the piston be hung by its ring on a nail, the syringe requires force to draw it down (just as much as to draw the piston up;) and if it be let go, it will spring up, unless loaded with at least 15 pounds for every square inch of its tranverse section, (see Fig. 2.)

4. But the most direct proof of the weight of the air is had by weighing a vessel empty of air, and then weighing it again when the air has been admitted; and this, as it is the most obvious consequence of its weight, has been asserted as long ago as the days of Aristotle. He says (περί οὐρανου, iv. 4.), That all bodies are heavy in their place except fire: even air is heavy; for a blown bladder is heavier than when it is empty. It is somewhat surprising that his followers should have gone into the opposite

opinion, while professing to maintain the doctrine of their leader. If we take a very large and limber bladder, and squeeze out the air very carefully, and weigh it, and then fill it till the wrinkles just begin to disappear, and weigh it again, we shall find no difference in the weight. But this is not Aristotle's meaning; because the bladder considered as a vessel, is equally full in both cases, its dimensions being changed. We cannot take the air out of a bladder without its immediately collapsing. But what would be true of a bladder would be equally true of any vessel. Therefore, take a round vessel A (Fig. 3.), fitted with a stopcock B, and syringe C. Fill the whole with water, and press the piston to the bottom of the syringe. Then keeping the cock open, and holding the vessel upright, with the syringe undermost, draw down the piston. The water will follow it by its weight, and leave part of the vessel empty. Now shut the cock, and again push up the piston to the bottom of the syringe; the water escapes through the piston valve, as will be explained afterward: then opening the cock, and again drawing down the piston, more water will come out of the vessel. Repeat this operation till all the water have come out. Shut the cock, unscrew the syringe, and weigh the vessel very accurately. Now open the cock, and admit the air, and weigh the vessel again, it will be found heavier than before, and this additional weight is the weight of the air which fills it; and it will be found to be 523 grains, about an ounce and a fifth avoirdupois, for every cubic foot that the vessel contains. Now, since a cubic foot of water would weigh 1000 ounces, this experiment would show that water is about 840 times heavier than air. The most accurate judgment of this kind of which we have met with an account, is that recorded by Sir George Shuckburgh, which is in the 67th vol. of the Philosophical Transactions, p. 560. From this it follows, that when the air is of the temperature 53, and barometer stands at 29½ inches, the air is 836 times lighter than water. But the experiment is not susceptible of sufficient accuracy for determining the exact weight of a cubic foot of air. Its weight is very small; and the vessel must be strong and heavy, so as to overload any balance that is sufficiently nice for the experiment.

To avoid this inconvenience, the whole may be weighed in water, first loading the vessel so as to make it preponderate an ounce or two in the water. By this means the balance will be loaded only with this small preponderancy. But even in this case there are considerable sources of error, arising from changes in the specific gravity of the water and other causes. The experiment has often been repeated with this view, and the air has been found at a medium to be about 840 times as light as water, but with great variations, as may be expected from its very heterogeneous nature, in consequence of its being the menstruum of almost every fluid, of all vapours, and even of most solid bodies; all which it holds in solution, forming a fluid perfectly transparent, and of very different density according to its composition. It is found, for instance, that perfectly pure air of the temperature of our ordinary summer, is considerably denser than when it has dissolved about half as much water as it can hold in that temperature; and that with this quantity of water the difference of density increases in proportion as the mass grows warmer, for damp air is more expansible by heat than dry air.

Such is the result of the experiment suggested by Aristotle, evidently proving the weight of the air; and yet, as has been observed, the Peripatetics, who profess to follow the *dictates* of Aristotle, uniformly refused it this property. It was a matter long debated among the philosophers of the last century. The reason was, that Aristotle, with that indistinctness and inconsistency which is observed in all his writings which relate to matters of fact and experience, assigns a different cause to many phenomena which any man led by common observation would ascribe to the weight of the air. Of this kind is the rise of water in pumps and syphons, which all the Peripatetics had for ages ascribed to something which they called *nature's abhorrence of a void*. Aristotle had asserted (for reasons not our business to adduce at present), that all nature was full of being, and that nature abhorred a void. He adduces many facts, in which it appears, that if not absolutely impossible, it is very difficult, and requires great force, to produce a space void of matter. When the operation of pumps and syphons came to be known, the philosophers of Europe (who had all embraced the Peripatetic doctrines) found in this fancied horror of a fancied mind (what else is this that nature abhors?) a ready solution of the phenomena. We shall state the facts, that every reader may see what kinds of reasoning were received among the learned not two centuries ago.

Pumps were then constructed in the following manner: A long pipe GB (Fig. 4.) was set in the water of the well A. This was fitted with a sucker or piston C, having a long rod CF, and was furnished with a valve B at the bottom, and a lateral pipe DE at the place of delivery, also furnished with a valve. The fact is, that if the piston be thrust down to the bottom, and then drawn up, the water will follow it; and upon the piston being again pushed down, the water shuts the valve B by its weight, and escapes, or is expelled at the valve E; and on drawing up the piston again the valve E is shut, the water again rises after the piston, and is again expelled at its next descent.

The Peripatetics explain all this by saying, that if the water did *not follow the piston* there would be a void between them. But nature abhors a void; or a void is impossible: therefore the water follows the piston. It is not worth while to criticise the wretched reasoning in this pretence to explanation. It is all overturned by one observation. Suppose the pipe shut at the bottom, the piston *can* be drawn up, and thus a void produced. No, say the Peripatetics; and they speak of certain spirits, effluvia, &c. which occupy the place. But if so, why needs the water rise? This therefore is not the cause of its ascent. It is a curious and important phenomenon.

The sagacious Galileo seems to have been the first who seriously ascribed this to the weight of the air. Many before him had supposed air heavy; and thus explained the difficulty of raising the board of bellows, or the piston of a syringe, &c. But he distinctly applies to this allowed weight of the air all the consequences of hydrostatical laws; and he reasons as follows:

The heavy air rests on the water in the cistern, and presses it with its weight. It does the same with the water in the pipe, and therefore both are on a level; but if the piston, after being in contact with the surface of the water, be drawn up, there is no longer any pressure on the surface of the water within the pipe; for the air now rests on the piston only, and thus occasions a difficulty in drawing it up. The water in the pipe, therefore, is in the same situation as if more water were poured into the cistern, that is, as much as would exert the same pressure on its surface as the air does. In this case we are certain that the water will be pressed into the pipe, and will raise up the water already in it, and follow it till it is equally high within and without. The same pressure of the air shuts the valve E during the descent of the piston. (See *Galileo's Discourses.*)

He did not wait for the very obvious objection, that if the rise of the water was the effect of the air's pressure, it would also be its measure, and would be raised and supported only to a certain height. He directly said so, and adduced this as a decisive experiment. If the horror of a void be the cause, says he, the water must rise to any height however great; but if it be owing to the pressure of the air, it will only rise till the weight of the water in the pipe is in equilibrio with the pressure of the air, according to the common laws of hydrostatics. And he adds, that this is well known; for it is a fact, that pumps will not *draw* water much above forty palms, although they may be made to *propel* or to *lift it* to any height. He then makes an assertion, which, if true, will be decisive. Let a very long pipe, shut at one end, be filled with water, and let it be erected perpendicularly with the close end uppermost, and a stopper in the other end, and then its lower orifice immersed into a vessel of water; the water will subside in the pipe upon removing the stopper, till the remaining column is in equilibrio with the pressure of the external air. This experiment he proposes to the curious; saying, however, that he thought it unnecessary, there being already such abundant proofs of the air's pressure.

It is probable that the cumbersomeness of the necessary apparatus protracted the making of this experiment. Another equally conclusive, and much easier, was made in 1642, after Galileo's death, by his zealous and learned disciple Toricelli. He filled a glass tube, close at one end, with mercury; judging, that if the support of the water was owing to the pressure of the air, and was the measure of this pressure, mercury would in like manner be supported by it, and this at a height which was also the measure of the air's pressure, and therefore 13 times less than water. He had the pleasure of seeing his expectation verified in the completest manner; the mercury descending in the tube AB (Fig. 5.) and finally settling at the height *f*B of 29¾ Roman inches: and he found, that when the tube was inclined, the point *f* was in the same horizontal plane with *f* in the upright tube, according to the received laws of hydrostatical pressure. The experiment was often repeated, and soon became famous, exciting great controversies among the philosophers about the possibility of a vacuum. About three years afterwards the same experiment was published at Warsaw in Poland, by Valerianus Magnus as his own suggestion and discovery: but it appears plain from the letters of Roberval, not only that Toricelli was prior, and that his experiment was the general topic of discussion among the curious; but also highly probable that Valerianus Magnus was informed of it when at Rome, and daily conversant with those who had seen it. He denies, however, even having heard of the name of Toricelli.

This was the era of philosophical ardour; and we think that it was Galileo's invention and immediate application of the telescope which gave it vigour. Discoveries of the most wonderful kind in the heavens, and which required no extent of previous knowledge to understand them, were thus put into the hands of every person who could purchase a spy-glass; while the high degree of credibility which some of the discoveries, such as the phases of Venus and the rotation and satellites of Jupiter, gave to the Copernican system, immediately set the whole body of the learned in motion. Galileo joined to his ardour a great extent of learning, particularly of mathematical knowledge and sound logic, and was even the first who formally united mathematics with physics; and his treatise on accelerated motion was the first, and a precious fruit of this union. About the years 1642 and 1644, we find clubs of gentlemen associated in Oxford and London for the cultivation of knowledge by experiment; and before 1655 all the doctrines of hydrostatics and pneumatics were familiar there, established upon experiment. Mr Boyle procured a coalition and correspondence of these clubs, under the name of the Invisible and Philosophical Society. In May 1658, Mr Hooke finished for Mr Boyle an air-pump, which had employed him a long time, and occasioned him several journeys to London for things which the workmen of Oxford could not execute. He speaks of this as a great improvement on Mr Boyle's own pump, which he had been using some time before. Boyle therefore must have invented his air-pump, and was not indebted for it to Schottus's account of Otto Guerick's, published in his (Schottus) *Mechanica Hydraulo-pneumatica* in 1657, as he asserts *(Techna Curiosa)*. The Royal Society of London arose in 1656 from the coalition of these clubs, after 15 years co-operation and correspondence. The Montmorine Society at Paris had subsisted nearly about the same time; for we find Paschal in 1648 speaking of the meetings in the Sorbonne College, from which we know that society originated.—Nuremberg, in Germany, was also a distinguished seminary of experimental philosophy. The magistrates, sensible of its valuable influence in manufactures, the source of the opulence and prosperity of their city, and many of them philosophers, gave philosophy a professed and munificent patronage, furnishing the philosophers with a copious apparatus, a place of assembly, and a fund for the expense of their experiments; so that this was the first academy of sciences out of Italy under the patronage of government. In Italy, indeed, there had long existed institutions of this kind. Rome was the centre of church-government, and the resort of all expectants for preferment. The clergy were the majority of the learned in all Christian nations, and particularly of the systematic philosophers. Each, eager to recommend himself to notice, brought forward every thing that was curious; and they were the willing vehicles of philosophical communication. Thus the experiments of Galileo and Toricelli were rapidly diffused by persons of rank, the dignitaries of the church, or by the monks, their obsequious servants. Perhaps the recent defection of England, and the want of a residing embassy at Rome, made her sometimes late in receiving or spreading philosophical researches, and was the cause that more was done there *proprio Marte*.

We hope to be excused for this digression. We were naturally led into it by the pretensions of Valerianus Magnus to originality in the experiment of the mercury supported by the pressure of the air. Such is the strength of national attachment, that there were not wanting some who found that Toricelli had borrowed his experiment from Honoratus Fabri, who had proposed and explained it in 1641; but whoever knows the writings of Toricelli, and Galileo's high opinion of him, will never think that he could need such helps. (See this surmise of Mounier in *Schott. Tech. Cur.* III. at the end.)

Galileo must be considered as the author of the experiment when he proposes it to be made. Valerianus Magnus owns himself indebted to him for the principle and the contrivance of the experiment. It is neither wonderful that many ingenious men, of one opinion, and instructed by Galileo, should separately hit on so obvious a thing; nor that Toricelli, his immediate disciple, his enthusiastic admirer, and who was in the habits of corresponding with him till his death, in 1642, should be the first to put it in practice. It became the subject of dispute from the national arrogance and self-conceit of some Frenchmen, who have always shown themselves disposed to consider their nation as at the head of the republic of letters, and cannot brook the concurrence of any foreigners. Roberval was in this instance, however, the champion of Toricelli; but those who know his controversies with the mathematicians of France at this time will easily account for this exception.

All now agree in giving Toricelli the honour of the *first* invention; and it universally passes by the name of the TORICELLIAN EXPERIMENT. The tube is called the TORICELLIAN TUBE; and the space left by the mercury is called the TORICELLIAN VACUUM, to distinguish it from the BOYLEAN VACUUM, which is only an extreme rarefaction.

The experiment was repeated in various forms, and with apparatus which enabled philosophers to examine several effects which the vacuum produced on bodies exposed in it. This was done by making the upper part of the tube terminate in a vessel of some capacity, or communicate with such a vessel, in which were included along with the mercury bodies on which the experiments were to be made. When the mercury had run out, the phenomena of these bodies were carefully observed.

An objection was made to the conclusion drawn from Torcelli's experiment, which appears formidable. If the Toricellian tube be suspended on the arm of a balance, it is found that the counterpoise must be equal to the weight both of the tube and of the mercury it contains. This could not be, say the objectors, if the mercury were supported by the air. It is evidently supported by the balance; and this gave rise to another notion of the cause different from the peripatetic *fuga vacui*: a suspensive force, or rather attraction, was assigned to the upper part of the tube.

But the true explanation of the phenomenon is most easy and satisfactory. Suppose the mercury in the cistern and tube to freeze, but without adhering to the tube, so that the tube could be freely drawn up and down. In this case the mercury is supported by the base, without any dependence on the pressure of the air; and the tube is in the same condition as before, and the solid mercury performs the office of a piston to this kind of syringe. Suppose the tube thrust down till the top of it touches the top of the mercury. It is evident that it must be drawn up in opposition to the pressure of the external air, and it is precisely similar to the syringe already mentioned. The weight sustained therefore by this arm of the balance is the weight of the tube and the downward pressure of the atmosphere on its top.

The curiosity of philosophers being thus excited by this very manageable experiment, it was natural now to try the original experiment proposed by Galileo. Accordingly Berti in Italy, Paschal in France, and many others in different places, made the experiment with a tube filled with water, wine, oil, &c. and all with the success which might be expected in so simple a matter: and the doctrine of the weight and pressure of the air was established beyond contradiction or doubt. All was done before the year 1648.—A very beautiful experiment was exhibited by Auzout, which completely satisfied all who had any remaining doubts.

A small box or phial EFGH (Fig. 6.) had two glass tubes, AB, CD, three feet long, inserted into it in such a manner as to be firmly fixed in one end, and to reach nearly to the other end. AB was open at both ends, and CD was close at D. This apparatus was completely filled with mercury, by unscrewing the tube AB, filling the box, and the hole CD; then screwing in the tube AB, and filling it: then holding a finger on the orifice A, the whole was inverted and set upright in the position represented in figure β, immersing the orifice A (now *a*) in a small vessel of quicksilver. The result was, that the mercury ran out at the orifice *a*, till its surface *m n* within the phial descended to the top of the tube *b a*. The mercury also began to descend in the tube *d c* (formerly DC) and run over into the tube *b a*, and ran out at *a*, till the mercury in *d c* was very near equal in the level with *m n*. The mercury descending in *b a* till it stood at *k*, 29½ inches above the surface *o p* of the mercury in the cistern, just as in the Toricellian tube.

The rationale of this experiment is very easy. The whole apparatus may first be considered as a Toricellian tube of an uncommon shape, and the mercury would flow out at *a*. But as soon as a drop of mercury comes out, leaving a space above *m n*, there is nothing to keep up the mercury in the tube *d c*. Its mercury therefore descends also; and running over into *b a*, continues to supply its expense till the tube *d c* is almost empty, or can no longer supply the waste of *b a*. The inner surface therefore falls as low as it can, till it is level with *b*. No more mercury can enter *b a*, yet its column is too heavy to be supported by the pressure of the air on the mercury in the cistern below; it therefore descends in *b a*, and finally settles at the height *k o*, equal to that of the mercury in the Toricellian tube.

The prettiest circumstance of the experiment remains. Make a small hole *g* in the upper cap of the box. The external air immediately rushes in by its weight, and now presses on the mercury in the box. This immediately raises the mercury in the tube *d c* to *l*, 29½ inches above *m n*. It presses on the mercury at *k* in the tube *b a*, balancing the pressure of the air in the cistern. The mercury in the tube therefore is left to the influence of its own weight, and it descends to the bottom. Nothing can be more apposite or decisive.

And thus the doctrine of the gravity and pressure of the air is established by the 'most unexceptionable evidence: and we are entitled to assume it as a statical principle, and to affirm *à priori* all its legitimate consequences.

And, in the first place, we obtain an exact measure of the pressure of the atmosphere. It is precisely equal to the weight of the column of mercury, of water, of oil, &c. which it can support; and the Toricellian tube, or others fitted up upon the same principle, are justly termed *baroscopes* and *barometers* with respect to the air. Now it is observed that water is supported at the height of 32 feet nearly: The weight of the column is exactly 2000 avoirdupois pounds in every square foot of base, or $13\frac{9}{10}$ on every square inch. The same conclusion very nearly may be drawn from the column of mercury, which is nearly 29½ inches high when in equilibrium with the pressure of the air. We may here observe, that the measure taken from the height of a column of water, wine, spirits, and the other fluids of considerable volatility, as chemists term it, is not so exact as that taken from mercury, oil, and the like. For it is observed, that the volatile fluids are converted by the ordinary heat of our climates into vapour when the confining pressure of the air is removed; and this vapour, by its elasticity, exerts a small pressure on the surface of the water, &c. in the pipe, and thus counteracts a small part of the external pressure; and therefore the column supported by the remaining pressure must be lighter, that is, shorter. Thus it is found, that rectified spirits will not stand much higher than is competent to a weight of 13 pounds on an inch, the elasticity of its vapour balancing about $\frac{1}{13}$ of the pressure of the air. We shall afterwards have occasion to consider this matter more particularly.

As the medium height of the mercury in the barometer is 29½ inches, we see that the whole globe sustains a pressure equal to the whole weight of a body of mercury of this height; and that all bodies on its surface sustain a part of this in proportion to their surfaces. An ordinary-sized man sustains a pressure of several thousand pounds. How comes it then that we are not sensible of a pressure which one should think enough to crush us together? This has been considered as a strong objection to the pressure of the air; for when a man is plunged a few feet under water, he is very sensible of the pressure. The answer is by no means so easy as is commonly imagined. We feel very distinctly the effects of removing this pressure from any part of the body. If any one will apply the open end of a syringe to his hand, and then draw up the piston, he will find his hand sucked into the syringe with great force, and it will give pain; and the soft part of the hand will swell into it, being pressed in by the neighbouring parts, which are subject to the action of the external air. If one lays his hand on the top of a long perpendicular pipe, such as a pump filled to the brim with water, which is at first prevented from running out by the valve below; and if the valve be then opened, so that the water descends, he will then find his hand so hard pressed to the top of the pipe that he cannot draw it away. But why do we only feel the *inequality* of pressure? There is a similar instance wherein we do not feel it, although we cannot doubt of its existence. When a man goes slowly to a great depth under water in a diving-bell, we know unquestionably that he is exposed to a new and very great pressure, yet he does not feel it. But these facts are not sufficiently familiar for general argument. The human body is a bundle of solids, hard or soft, filled or mixed with fluids, and there are few or no parts of it which are empty. All communicate either by vessels or pores; and the whole surface is a sieve through which the insensible perspiration is performed. The whole extended surface of the lungs is open to the pressure of the atmosphere; every thing is therefore in equilibrio; and if free or speedy access be given to every part, the body will not be damaged by the pressure, however great, any more than a wet sponge would be deranged by plunging it any depth in water. The pressure is instantaneously diffused by means of the incompressible fluids with which the parts are filled; and if any parts are filled with air or other compressible fluids, these are compressed till their elasticity again balances the pressure. Besides, all our fluids are acquired slowly, and gradually mixed with that proportion of air which they can dissolve or contain. The whole animal has grown up in this manner from the first vital atom of the embryo. For such reasons the pressure can occasion no change of shape by squeezing together the flexible parts; nor any obstruction by compressing the vessels or pores. We cannot say what would be felt by a man, were it possible that he could have been produced and grown up *in vacuo*, and then subjected to the compression. We even know that any sudden and considerable change of general pressure is very severely felt. Persons in a diving-bell have been almost killed by letting them down or drawing them up too suddenly. In drawing up, the elastic matters within have suddenly swelled, and not finding an immediate escape, have burst the vessels. Dr Halley experienced this, the blood gushing out from his ears by the expansion of air contained in the internal cavities of this organ, from which there are but very slender passages.

A very important observation recurs here: the pressure of the atmosphere is variable. This was observed almost as soon as philosophers began to attend to the barometer. Paschal observed it in France, and Descartes observed it in Sweden in 1650. Mr Boyle and others observed it in England in 1656. And before this, observers, who took notice of the concomitancy of these changes of aerial pressure with the state of the atmosphere, remarked, that it was generally greatest in winter and in the night; and certainly most variable during winter and in the northern regions. Familiar now with the weight of the air, and considering it as the vehicle of the clouds and vapours, they noted with care the connexion between the weather and the pressure of the air, and found that a great pressure of the air was generally accompanied with fair weather, and a diminution of it with rain and mists. Hence the barometer came to be considered as an index not only of the present state of the air's weight, but also as indicating by its variations changes of weather. It became a WEATHER-GLASS, and continued to be anxiously observed with this view. This is an important subject, and will afterwards be treated in some detail.

In the next place, we may conclude that the pressure of the air will be different in different places, according to their elevation above the surface of the ocean: for if air be an heavy fluid, it must press in some proportion according to its perpendicular height. If it be a homogeneous fluid of equal density and weight in all its parts, the mercury in the cistern of a barometer must be pressed precisely in proportion to the depth to which that cistern is immersed in it; and as this pressure is exactly measured by the height of the mercury in the tube, the height of the mercury in the Toricellian tube must be exactly proportional to the depth of the place of observation under the surface of the atmosphere.

The celebrated Descartes first entertained this thought (Epist. 67. of Pr. III.), and soon after him Paschal. His occupation in Paris not permitting him to try the justness of his conjecture, he requested Mr Perrier, a gentleman of Clermont in Auvergne, to make the experiment by observing the height of the mercury at one and the same time at Clermont and on the top of a very high mountain in the neighbourhood. His letters to Mr Perrier in 1647 are still extant. Accordingly Mr Perrier, in September 1648, filled two equal tubes with mercury, and observed the heights of both to be the same, viz. $26\frac{7}{24}$ inches, in the garden of the convent of the Friars Minims, situated in the lowest part of Clermont. Leaving one of them there, and one of the fathers to observe it, he took the other to the top of Puy de Dome, which was elevated nearly 500 French fathoms above the garden. He found its height to be $23\frac{2}{24}$ inches. On his return to the town, in a place called *Font de l'Arbre*, 150 fashoms above the garden, he found it 25 inches; when he returned to the garden it was again $26\frac{7}{24}$, and the person set to watch the tube which had been left said that it had not varied the whole day. Thus a difference of elevation of 3000 French feet had occasioned a depression of $3\frac{1}{8}$ inches; from which it may be concluded, that $3\frac{1}{8}$ inches of mercury weighs as much as 3000 feet of air, and one-tenth of an inch of mercury as much as 96 feet of air. The next day he found, that taking the tube to the top of a steeple 120 feet high made a fall of one-sixth of an inch. This gives 72 feet of air for one-tenth of an inch of mercury; but ill agreeing with the former experiment. But it is to be observed, that a very small error of observation of the barometer would correspond to a great difference of elevation, and also that the height of the mountain had not been measured with any precision. This has been since done

(Mem. Acad. par. 1703), and found to be 529 French toises.

Paschal published an account of this great experiment (*Grande Exp. sur la Pesanteur de l' Air*), and it was quickly repeated in many places of the world. In 1653, it was repeated in England by Dr Power (Power's Exper. Phil.); and in Scotland, in 1661, by Mr Sinclair, professor of philosophy in the university of Glasgow, who observed the barometer at Lanark, on the top of mount Tintock in Clydesdale, and on the top of Arthur's Seat at Edinburgh. He found a depression of two inches between Glasgow and the top of Tintock, three quarters of an inch between the bottom and top of Arthur's Seat, and $\frac{1}{16}$ of an inch at the cathedral of Glasgow on the height of 126 feet. See Sinclair's *Ars Nova et Magna Gravitatis et Levitatis*; Sturmii *Collegium Experimentale*, and Schotti *Technica Curiosa.*

Hence we may derive a method of measuring the heights of mountains. Having ascertained with great precision the elevation corresponding to a fall of one-tenth of an inch of mercury, which is nearly 90 feet, we have only to observe the length of the mercurial column at the top and bottom of the mountain, and to allow 90 feet for every tenth of an inch. Accordingly this method has been practised with great success: but it requires an attention to many things not yet considered; such as the change of density of the mercury by heat and cold; the changes of density of air, which are much more remarkable from the same causes; and, above all, the changes of the density of air from its compressibility; a change immediately connected with or dependent on the very elevation we wish to measure. Of all these afterwards.

These observations give us the most accurate measure of the density of the air and its specific gravity. This is but vaguely though directly measured by weighing air in a bladder or vessel. The weight of a manageable quantity is so small, that a balance sufficiently ticklish to indicate even very sensible fractions of it is overloaded by the weight of the vessel which contains it, and ceases to be exact: and when we take Bernoulli's ingenious method of suspending it in water, we expose ourselves to great risk of error by the variation of the water's density. Also it must necessarily be humid air which we can examine in this way: but the proportion of an elevation in the atmosphere to the depression of the column of mercury or other fluid, by which we measure its pressure, gives us at once the proportion of this weight or their specific gravity. Thus, since it is found that in such a state of pressure that the barometer stands at 30 inches, and the thermometer at 32°, 87 feet of rise produces one-tenth of an inch of fall in the barometer, the air and the mercury being both of the freezing temperature, we must conclude that mercury is 10,440 times heavier or denser than air. Then, by comparing mercury and water, we get $\frac{1}{801}$ nearly, for the density of air relative to water: but this varies so much by heat and moisture, that it is useless to retain any thing more than a general notion of it; nor is it easy to determine whether this method or that by actual weighing is preferable. It is extremely difficult to observe the height of the mercury in the barometer nearer than $\frac{1}{200}$ of an inch; and this will produce a difference of even five feet, or $\frac{1}{20}$ of the whole. Perhaps this is a greater proportion than the error in weighing.

From the same experiments we also derive some knowledge of the height of the aerial covering which surrounds our globe. When we raise our barometer 87 feet above the surface of the sea, the mercury falls about one-tenth of an inch in the barometer: therefore if the barometer shows 30 inches at the sea-shore, we may expect that, by raising it 300 times 87 feet, or five miles, the mercury in the tube will descend to the level of the cistern, and that this is the height of our atmosphere. But other appearances lead us to suppose a much greater height. Meteors are seen with us much higher than this, and which yet give undoubted indication of being supported by our air. There can be little doubt, too, that the visibility of the expanse above us is owing to the reflection of the sun's light by our air. Were the heavenly spaces perfectly transparent, we should no more see them than the purest water through which we see other objects; and we see *them* as we see water tinged with milk or other fæculæ. Now it is easy to show, that the light which gives us what is called twilight must be reflected from the height of at least 50 miles; for we have it when the sun is depressed 18 degrees below our horizon.

A little attention to the constitution of our air will convince us, that the atmosphere must extend to a much greater height than 300 times 87 feet. We see from the most familiar facts that it is compressible; we can squeeze it in an ox bladder. It is also heavy; pressing on the air in this bladder with a very great force, not less than 1500 pounds. We must therefore consider it as in a state of compression, existing in smaller room than it would assume if it were not compressed by the incumbent air. It must therefore be in a condition something resembling that of a quantity of fine carded wool thrown loosely into a deep pit; the lower strata carrying the weight of the upper strata, and being compressed by them; and so much the more compressed as they are further down, and only the upper stratum in its unconstrained and most expanded state. If we shall suppose this wool thrown in by a hundred weight at a time, it will be divided into strata of equal weights, but of unequal thickness; the lowest being the thinnest, and the superior strata gradually increasing in thickness. Now, suppose the pit filled with air, and reaching to the top of the atmosphere, the *weights* of all the strata above any horizontal plane in it is measured by the height of the mercury in the Torricellian tube placed in that plane; and one-tenth of an inch of mercury is just equal to the weight of the lowest stratum 87 feet thick: for on raising the tube 87 feet from the sea, the surface of the mercury will descend one-tenth of an inch. Raise the tube till the mercury fall another tenth: this stratum must be more than 87 feet thick; how much more we cannot tell, being ignorant of the law of the air's expansion. In order to make it fall a third tenth, we must raise it through a stratum still thicker; and so on continually.

All this is abundantly confirmed by the very first experiment made by the order and directions of Paschal: for by carrying the tube from the garden of the convent to a place 150 fathoms higher, the mercury fell $1\frac{7}{24}$ inches, or 1,2917; which gives about 69 feet 8 inches of aerial stratum for $\frac{1}{10}$ of an inch of mercury; and by carrying it from thence to a place 350 fathoms higher, the mercury fell $1\frac{22}{24}$, or 1,9167 inches, which gives 109 feet 7 inches for $\frac{1}{10}$ of an inch of mercury. These experiments were not accurately made; for at that time the philosophers, though zealous, were but *scholars* in the *science* of experimenting, and *novices* in the art. But the results abundantly show this general truth, and they are completely confirmed by thousands of subsequent observations. It is evident from the whole tenor of them, that the strata of air decrease in density as we ascend through the atmosphere; but it remained to be discovered what is the force of this decrease, that is, the law of the air's expansion. Till this be done we can say nothing about the constitution of our atmosphere; we cannot tell in what manner it is fittest for raising and supporting the exhalations and vapours which are continually arising from the inhabited regions; not as an excrementitious waste, but to be supported, perhaps manufactured, in that vast laboratory of nature, and to be returned to us in beneficent showers. We cannot use our knowledge for the curious, and frequently useful, purpose of measuring the heights of mountains and taking the levels of extensive regions; in short, without an accurate knowledge of this, we can hardly acquire any acquaintance with those mechanical properties which distinguish air from those liquids which circulate here below.

Having therefore considered at some length the leading consequences of the air's fluidity and gravity, let us consider its compressibility with the same care; and then, combining the agency of both, we shall answer all the purposes of philosophy, discover the laws, explain the phenomena of nature, and improve art. We proceed therefore to consider a little the phenomena which indicate and characterise this other property of the air. All fluids are elastic and compressible as well as air; but in them the compressibility makes no figure, or does not interest us while we are considering their pressures, motions, and impulsions. But in air the compressibility and expansion draw our chief attention, and make it a proper representative of this class of fluids.

Nothing is more familiar than the compressibility of air. It is seen in a bladder filled with it, which we can forcibly squeeze into less room; it is seen in a syringe, of which we can push the plug farther and farther as we increase the pressure.

But these appearances bring into view another, and the most interesting, property of air, viz. its *elasticity*. When we have squeezed the air in the bladder or syringe into less room, we find that the force with which we *compressed* it is necessary to *keep it in this bulk*; and that if we cease to press it together, it will swell out and regain its natural dimensions. This distinguishes it essentially from such a body as a mass of flour, salt, or such like, which remain in the compressed state to which we reduce them.

There is therefore something which opposes the compression different from the simple impenetrability of the air: there is something that opposes mechanical force; there is something too which produces motion, not only resisting compression, but pushing back the compressing body, and communicating motion to it. As an arrow is gradually accelerated by the bow-string pressing it forward, and at the moment of its discharge is brought to a state of rapid motion; so the ball from a pop-gun, or wind-gun, is gradually accelerated along the barrel by the pressure of the air during its expansion from its compressed state, and finally quits it with an accumulated velocity. These two motions are indications perfectly similar of the elasticity of the bow and of the air.

Thus it appears that air is heavy and elastic. It needs little consideration to convince us in a vague manner that it is fluid. The ease with which it is penetrated, and driven about in every direction, and the motion of it in pipes and channels, however crooked and intricate, entitle it to this character. But before we can proceed to deduce consequences from its fluidity, and to offer them as a true account of what will happen in these circumstances, it is necessary to exhibit some distinct and simple case, in which the characteristic mechanical property of a fluid is clearly and unequivocally observed in it. That property of fluids from which all the laws of hydrostatics and hydraulics are derived with strictest evidence is, that any pressure applied to any part of them is propagated through the whole mass in every direction; and that, in consequence of this diffusion of pressure, any two external forces can be put in equilibrio by the interposition of a fluid, in the same way as they can be put in equilibrio by the intervention of any mechanical engine.

Let a close vessel ABC (Fig. 7.), of any form, have two upright pipes EDC, GFB, inserted into any parts of its top, sides, or bottom, and let water be poured into them, so as to stand in equilibrio with the horizontal surfaces at E, D, G, F, and let D *d*, F *f*, be horizontal lines, *it will be found* that the height of the column E *d*, is sensibly equal to that of the column G . This is a fact universally observed in whatever way the pipes are inserted.

Now the surface of the water at D is undoubtedly pressed upwards with a force equal to a column of water, having its surface for its base, and E *d* for its height; it is therefore prevented from rising by some opposite force. This can be nothing but the elasticity of the confined air pressing it down. The very same thing must be said of the surface at F; and thus there are two external pressures at D and F set in equilibrio by the interposition of air. The force exerted on the surface D, by the pressure of the column E *d*, is therefore propagated to the surface at F; and thus air has this characteristic mark of fluidity.

In this experiment the *weight* of the air is insensible when the vessel is of small size, and has no sensible share in the pressure reaching at D and F. But if the elevation of the point F above D is very great, the column E *d will be observed* sensibly to exceed the column G *f*. Thus if F be 70 feet higher than D, E *d* will be an inch longer than the column G *f*: for in this case there is reacting at D, not only the pressure propagated from F, but also the weight of a column of air, having the surface at D for its base and 70 feet high. This is equal to the weight of a column of water one inch high.

It is by this propagation of pressure, this *fluidity*, that the pellet is discharged from a child's pop-gun. It sticks fast in the muzzle; and he forces in another pellet at the other end, which he presses forward with the rammer, condensing the air between them, and thus propagating to the other pellet the pressure which he exerts, till the friction is overcome, and the pellet is discharged by the air expanding and following it.

There is a pretty philosophical plaything which illustrates this property of air in a very perspicuous manner, and which we shall afterwards have occasion to consider as converted into a most useful hydraulic machine. This is what is usually called *Hero's fountain*, having been invented by a Syracusan of that name. It consists of two vessels KLMN (Fig 8.), OPQR, which are close on all sides. A tube AB, having a funnel a-top, passes through the uppermost vessel without communicating with it, being soldered into its top and bottom. It also passes through the top of the under-vessel, where it is also soldered, and reaches almost to its bottom. This tube is open at both ends. There is another open tube ST, which is soldered into the top of the under-vessel and the bottom of the upper-vessel, and reaches almost to its top. These two tubes serve also to support the upper-vessel. A third tube GF is soldered into the top of the upper vessel, and reaches almost to its bottom. This tube is open at both ends, but the orifice G is very small. Now suppose the uppermost vessel filled with water to the height EN, E*e* being its surface a little below T. Stop the orifice G with the finger, and pour in water at A. This will descend through AB, and compress the air in OQRP into less room. Suppose the water in the under vessel to have acquired the surface C *c*, the air which formerly occupied the whole of the spaces OPQR and KL *e* E will now be contained in the spaces *o* P *o* C and KL *e* E; and its elasticity will be in equilibrio with the weight of the column of water, whose base is the surface E *e*, and whose height is A *c*. As this pressure is exerted in every part of the air, it will be exerted on the surface E *e* of the water of the upper vessel; and if the pipe FG were continued upwards, the water would be supported in it to an height *e* H above E *e*, equal to A *c*. Therefore if the finger be now taken from off the orifice G, the water will spout up to the same height as if it had been immediately forced out by a column of water A *c* without the intervention of the air, that is, nearly to H. If instead of the funnel at A, the vessel have a brim which will cause the water discharged at G to run down the pipe AB, this fountain will play till all the water in the upper vessel is expended. The operation of this second fountain will be better understood from Fig. 9. which an intelligent reader will see is perfectly equivalent to Fig. 8. A very powerful engine for raising water upon this principle has long been employed in the Hungarian mines; where the pipe AB is about 200 feet high, and the pipe FG about 120; and the condensation is made in the upper vessel, and communicated to the lower, at the bottom of the mine, by a long pipe.

We may now apply to air all the laws of hydrostatics and hydraulics, in perfect confidence that their legitimate consequences will be observed in all its situations. We shall in future substitute, in place of any force acting on a surface of air, a column of water, mercury, or any other fluid whose weight is equal to this force: and as we know distinctly from theory what will be the consequences of this hydrostatic pressure, we shall determine *à priori* the phenomena in air; and in cases where the theory does not enable us to say with precision what is the effect of this pressure, experience informs us in the case of water, and analogy enables us to transfer this to air. We shall find this of great service in some cases, which otherwise are almost desperate in the present state of our knowledge.

From such familiar and simple observations and experiments, the fluidity, the heaviness, and elasticity, are discovered of the substance with which we are surrounded, and which we call *air*. But to understand these properties, and completely to explain their numerous and important consequences, we must call in the aid of more refined observations and experiments which even this scanty knowledge of them enablesus to make; we must contrive some methods of producing with precision any degree of condensation or rarefaction, of employing or excluding the gravitating pressure of air, and of modifying at pleasure the action of all its mechanical properties.

Nothing can be more obvious than a method of compressing a quantity of air to any degree. Take a cylinder or prismatic tube AB (Fig. 10.) shut at one end, and fit it with a piston or plug C, so nicely that no air can pass by its sides. This will be best done in a cylindric tube by a turned stopper, covered with oiled leather, and fitted with a long handle CD. When this is thrust down, the air which formerly occupied the whole capacity of the tube is *condensed* into less room. The force necessary to produce any degree of compression may be concluded from the weight necessary for pushing down the plug to any depth. But this instrument leaves us little opportunity of making interesting experiments on or in this condensed air; and the force required to make any degree of compression cannot be measured with much accuracy; because the piston must be very close, and have great friction, in order to be sufficiently tight: and as the compression is increased, the leather is more squeezed to the side of the tube; and the proportion of the external force, which is employed merely to overcome this variable and uncertain friction, cannot be ascertained with any tolerable precision. To get rid of these imperfections, the following addition may be made to the instrument, which then becomes what is called the *condensing syringe*.

The end of the syringe is perforated with a very small hole *ef*; and being externally turned to a small cylinder, a narrow slip of bladder, or of thin leather, soaked in a mixture of oil and tallow, must be tied over the hole. Now let us suppose the piston pushed down to the bottom of the barrel to which it applies close; when it is drawn up to the top, it leaves a void behind, and the weight of the external air presses on the slip of bladder, which therefore claps close to the brass, and thus performs the part of a valve, and keeps it close, so that no air can enter.

But the piston having reached the top of the barrel, a hole F in the side of it is just below the piston, and the air rushes through this hole and fills the barrel. Now push the piston down again, it immediately passes the hole F, and no air escapes through it; it therefore forces open the valve at *f*, and escapes while the piston moves to the bottom.

Now let E be any vessel, such as a glass bottle, having its mouth furnished with a brass cap firmly cemented to it, having a hollow screw which fits a solid screw *p o*, turned on the cylindric nozzle of the syringe. Screw the syringe into this cap, and it is evident that the air forced out of the syringe will be accumulated in this vessel: for upon drawing up the piston the valve *f* always shuts by the elasticity or expanding force of the air in E; and on pushing it down again, the valve will open as soon as the piston has got so far down that the air in the lower part of the barrel is more powerful than the air already in the vessel. Thus at every stroke an additional barrelful of air will be forced into the vessel E; and it will be found, that after every stroke the piston must be farther pushed down before the valve will open. It cannot open till the pressure arising from the elasticity of the air condensed in the barrel is superior to the elasticity of the air condensed in the vessel; that is, till the condensation of the first, or its density, is *somewhat* greater than that of the last, in order to overcome the straining of the valve on the hole and the sticking occasioned by the clammy matter employed to make it air-tight.

Sometimes the syringe is constructed with a valve in the piston. This piston, instead of being of one piece and solid, consists of two pieces perforated. The upper part *i k n m* is connected with the rod or handle, and has its lower part turned down to a small cylinder, which is screwed into the lower part *k l o n*; and has a perforation *g h* going up in the axis, and terminating in a hole *h* in one side of the rod, a piece of oiled leather is strained across the hole *g*. When the piston is drawn up and a void left below it, the weight of the external air forces it through the hole *h g*, opens the valve *g*, and fills the barrel. Then, on pushing down the piston, the air being squeezed into less room, presses on the valve *g*, shuts it; and none escaping through the piston, it is gradually condensed as the piston descends till it opens the valve *f*, and is added to that already accumulated in the vessel E.

Having in this manner forced a quantity of air into the vessel E, we can make many experiments in it in this state of condensation. We are chiefly concerned at present with the effect which this produces on its elasticity. We see this to be greatly increased; for we find more and more force required for introducing every successful barrelful. When the syringe is unscrewed, we see the air rush out with great violence, and every indication of great expanding force. If the syringe be connected with the vessel E in the same manner as the syringe in No 17, viz. by interposing a stop-cock B between them (see Fig. 3.), and if this stop-cock have a pipe at its extremity, reaching near to the bottom of the vessel, which is previously half filled with water, we can observe distinctly when the elasticity of the air in the syringe exceeds that of the air in the receiver: for the piston must be pushed down a certain length before the air from the syringe bubbles up through the water, and the piston must be further down at each successive stroke before this appearance is observed. When the air has thus been accumulated in the receiver, it presses the sides of it outward, and will burst it if not strong enough. It also presses on the surface of the water; and if we now shut the cock, unscrew the syringe, and open the cock again, the air will force the water through the pipe with great velocity, causing it to rise in a beautiful jet. When a metal receiver is used, the condensation may be pushed to a great length, and the jet will then rise to a great height; which gradually diminishes as the water is expended and room given to the air to expand itself. See the figure.

We judge of the condensation of air in the vessel E by the number of strokes and the proportion of the capacity of the syringe to that of the vessel. Suppose the first to be one-tenth of the last; then we know, that after 10 strokes the quantity of air in the vessel is doubled, and therefore its density double, and so on after any number of strokes. Let the capacity of the syringe (when the piston is drawn to the top) be a, and that of the vessel be b, and the number of strokes be n, the density of the air in the vessel will be $\frac{b+na}{b}$, or $1+\frac{na}{b}$.

But this is on the supposition that the piston accurately fills the barrel, the bottom of the one applying close to that of the other, and that no force is necessary for opening either of the valves: but the first cannot be ensured, and the last is very far from being true. In the construction now described, it will require at least one-twentieth part of the ordinary pressure of the air to open the piston valve: therefore the air which gets in will want at least this proportion of its complete elasticity; and there is always a similar part of the elasticity employed in opening the nozzle valve. The condensation therefore is never nearly equal to what is here determined.

It is accurately enough measured by a gage fitted to the instrument. A glass tube GH of a cylindric bore, and close at the end, is screwed into the side of the cap on the mouth of the vessel E. A small drop of water or mercury is taken into this tube by warming it a little in the hand, which expands the contained air, so that when the open end is dipped into water, and the whole allowed to cool, the water advances a little into the tube. The tube is furnished with a scale divided into small equal parts, numbered from the close end of the tube. Since this tube communicates with the vessel, it is evident that the condensation will force the water along the tube, acting like a piston on the air beyond it, and the air in the tube and vessel will always be of one density. Suppose the number at which the drop stands before the condensation is made to be c, and that it stands at d when the condensation has attained the degree required, the density of the air in the remote end of the gage, and consequently in the vessel, will be $\frac{c}{d}$.

Sometimes there is used any bit of tube close at one end, having a drop of water in it, simply laid into the vessel E, and furnished or not with a scale: but this can only be used with glass vessels, and these are too weak to resist the pressure arising from great condensation. In such experiments metalline vessels are used, fitted with a variety of apparatus for different experiments. Some of these will be occasionally mentioned afterwards.

It must be observed in this place, that very great condensations require great force, and therefore small syringes. It is therefore convenient to have them of various sizes, and to begin with those of a larger diameter, which operate more quickly; and when the condensation becomes fatiguing, to change the syringe for a smaller.

For this reason, and in general to make the condensing apparatus more convenient, it is proper to have a stop-cock interposed between the syringe and the vessel, or, as it is usually called, the receiver. This consists of a brass pipe, which has a well-ground cock in its middle, and has a hollow screw at one end, which receives the nozzle screw of the syring, and a solid screw at the other end, which fits the screw of the receiver. See Fig. 3.

By these gages, or contrivances similar to them, we have been able to astertain very great degrees of condensation in the course of some experiments. Dr Hales found that when dry wood was put into a strong vessel, which it almost filled, and the remainder was filled with water, the swelling of the wood, occasioned by its imbibation of water, condensed the air of his gage into the thousandth of its original bulk. He found that pease treated in the same manner generated elastic air, which pressing on the air in the gage condensed it into the fifteen hundredth part of its bulk. This is the greatest condensation that has been ascertained with precision, although in other experiments it has certainly been carried much farther; but the precise degree could not be ascertained.

The only use to be made of this observation at present is, that since we have been able to exhibit air in a density a thousand times greater than the ordinary density of the air we breathe, it cannot, as some imagine, be only a different form of water; for in this state it is as dense or denser than water, and yet retains its great expansibility.

Another important observation is, that in every state of density in which we find it, it retains its perfect fluidity, transmitting all pressures which are applied to it with undiminished force, as appears by the equality constantly observed between the opposing columns of water or other fluid by which it is compressed, and by the facility with which all motions are performed in it in the most compressed states in which we can make observations of this kind. This fact is totally incompatible with the opinion of those who ascribe the elasticity of air to the springy ramified structure of its particles, touching each other like so many pieces of sponge or foot-balls. A collection of such particles might indeed be pervaded by solid bodies with considerable ease, if they were merely touching each other, and not subjected to any external pressure. But the moment such pressure is exerted, and the assemblage squeezed into a smaller space, each pressure on its adjoining particles: they are individually compressed, flattened in their touching surfaces, and *before the density is doubled* they are squeezed into the form of perfect cubes, and compose a mass, which may indeed propagate pressure from one place to another in an imperfect manner, and with great diminution of its intensity, but will no more be fluid than a mass of soft clay. It will be of use to keep this observation in mind.

We have seen that air is heavy and compressible, and might now proceed to deduce in order the explanation of the appearances consequent on each of these properties. But, as has been already observed, the elasticity of air modifies the effects of its gravity so remarkably, that they would be imperfectly understood if both qualities were not combined in our consideration of either. At any rate, some farther consequences of its elasticity must be considered, before we understand the means of varying at pleasure the effects of its gravity.

Since air is heavy, the lower strata of a mass of air must support the upper; and, being compressible, they must be condensed by their weight. In this state of compression the elasticity of the lower strata of air acts in opposition to the weight of the incumbent air, and balances it. There is no reason which should make us suppose that its expanding force belongs to it only when in such a state of compression. It is more probable, that, if we could free it from this pressure, the air would expand itself into still greater bulk. This is most distinctly seen in the following experiment:

Into the cylindric jar ABCD (Fig. 11.), which has a small hole in its bottom, and is furnished with an air-tight piston E, put a small flaccid bladder, having its mouth tied tight with a string. Having pushed the piston near to the bottom, and noticed the state of the bladder, stop up the hole in the bottom of the jar with the finger, and draw up the piston, which will require a considerable force. You will observe the bladder swell out, as if air had been blown into it; and it will again collapse on allowing the piston to descend. Nothing can be more unexceptionable than the conclusion from this experiment, that ordinary air is in a state of compression, and that its elasticity is not limited to this state. The bladder being flaccid, shows that the included air is in the same state with the air which surrounds it; and the same must be affirmed of it while it swells but still remains flaccid. We must conclude that the whole air within the vessel expands, and continues to fill it, when its capacity has been enlarged. And since this is observed to go on as long as we give it more room, we conclude, that by such experiments we have not yet given it so much room as it can occupy.

It was a natural object of curiosity to discover the limits of this expansion; to know what was the natural unconstrained bulk of a quantity of air, beyond which it would not expand though all external compressing force were removed. Accordingly philosophers constructed instruments for *rarefying* the air. The common water-pump had been long familiar, and appeared very proper for this purpose. The most obvious is the following:

Let the barrel of the syringe AB (Fig. 12.) communicate with the vessel V, with a stop-cock C between them. Let it communicate with the external air by another orifice D, in any convenient situation, also furnished with a stop-cock. Let this syringe have a piston very accurately fitted to it, so as to touch the bottom all over when pushed down, and have no vacancy about the sides.

Now suppose the piston at the bottom, the cock C open, and the cock D shut, draw the piston to the top. The air which filled the vessel V will expand so as to fill both that vessel and the barrel AB; and as no reason can be given to the contrary, we must suppose that the air will be uniformly diffused through both. Calling V and B the capacity of the vessel and barrel, it is plain that the bulk of the air will now be V + B; and since the quantity of matter remains the same, and the density of a fluid is as its quantity of matter directly and its bulk inversely, the density of the expanded air will be $\frac{V}{V+B}$, the density of common air being 1: for $V+B : V = 1 : \frac{V}{V+B}$.

The piston requires force to raise it, and it is raised in opposition to the pressure of the incumbent atmosphere; for this had formerly been balanced by the elasticity of the common air: and we conclude from the fact, *that force is required to raise the piston*, that the elasticity of the expanded air is less than that of air in its ordinary state; and an accurate observation of the force necessary to raise it would show how much the elasticity is diminished. When therefore the piston is let go, it will descend as long as the pressure of the atmosphere exceeds the elasticity of the air in the barrel; that is, till the air in the barrel is in a state of ordinary density. To put it further down will require force, because the air must be compressed in the barrel; but if we now open the cock D, the air will be expelled through it, and the piston will reach the bottom.

Now shut the *discharging cock* D, and open the cock C, and draw up the piston. The air which occupied the space V, with the density $\frac{V}{V+B}$, will now occupy the space V + B, if it expands so far. To have its density D, say, As its present bulk V + B is to its former bulk V, so is its former density $\frac{V}{V+B}$ to its new density; which will therefore be $\frac{V \times V}{\overline{V+B} \times \overline{V+B}}$, or $\overline{\frac{V}{V+B}}\Big|^2$.

It is evident, that if the air continues to expand, the density of the air in the vessel after the third drawing up of the piston will be $\overline{\frac{V}{V+B}}\Big|^3$, after the fourth it will be

$\left.\overline{\frac{V}{V+B}}\right|^{4}$, and after any number of strokes n will be $\left.\overline{\frac{V}{V+B}}\right|^{n}$. Thus, if the vessel is four times as large as the barrel, the density after the fifth stroke will be $\frac{1024}{3125}$, nearly $\frac{1}{3}$ of its ordinary density.

On the other hand, the number n of strokes necessary for reducing air to the density D is $\frac{\text{Log } D}{\text{Log } V - \text{Log } (V+B)}$.

Thus we see that this instrument can never abstract the whole air in consequence of its expansion, but only rarefy it continually as long as it continues to expand; nay, there is a limit beyond which the rarefaction cannot go. When the piston has reached the bottom, there remains a small space between it and the cock C filled with common air. When the piston is drawn up, this small quantity of air expands, and also a similar quantity in the neck of the other cock; and no air will come out of the receiver V till the air expanded in the barrel is of a smaller density than the air in the receiver. This circumstance evidently directs us to make these two spaces as small as possible, or by some contrivance to fill them up altogether. Perhaps this may be done effectually in the following manner:

Let BE (Fig. 13.) represent the bottom of the barrel, and let the circle HKI be the section of the key of the cock, of a large diameter, and place it as near to the barrel as can be. Let this communicate with the barrel by means of an hole FG widening upwards, as the frustum of a hollow obtuse cone. Let the bottom of the piston *bfhge* be shaped so as to fit the bottom of the barrel and this hole exactly. Let the cock be pierced with two holes. One of them, HI, passes perpendicularly through its axis, and forms the communication between the receiver and barrel. The other hole, KL, has one extremity K on the same circumference with H, so that when the key is turned a fourth part round, K will come into the place of H:

but this hole is pierced obliquely into the key, and thus keeps clear of the hole HI. It goes no further than the axis, where it communicates with a hole bored along the axis and terminating at its extremity. This hole forms the communication with the external air, and serves for discharging the air in the barrel. (A side view of the key is seen in Fig. 14.) Fig. 12. shows the position of the cock while the piston is moving upwards, and Fig. 14. shows its position while the piston is moving downwards. When the piston has reached the bottom, the conical piece *f h g* of the piston, which may be of firm leather, fills the hole FHG, and therefore completely expels the air from the barrel. The canal KL *l* of the cock contains air of the common density; but this is turned aside into the position KL (Fig. 13.), while the piston is still touching the cock. It cannot expand into the barrel during the ascent of the piston. In place of it, the perforation HLI comes under the piston, filled with air that had been turned aside with it when the piston was at the top of the barrel, and therefore of the same density with the air of the receiver. It appears, therefore, that there is no limit to the rarefaction as long as the air will expand.

This instrument is called an EXHAUSTING SYRINGE. It is more generally made in another form, which is much less expensive, and more convenient in its use. Instead of being furnished with *cocks* for establishing the communications and shutting them, as is necessary, it has *valves* like those of the condensing syringe, but opening in the opposite direction. It is thus made:

The pipe of communication or conduit MN (Fig. 15.), has a male screw in its extremity, and over this is tied a slip of bladder or leather M. The lower half of the piston has also a male screw on it, covered at the end with a slip of bladder O. This is screwed into the upper half of the piston, which is pierced with a hole H coming out of the side of the rod.

Now suppose the syringe screwed to the conducting pipe, and that screwed into the receiver V and the piston at the bottom of the barrel. When the piston is drawn up, the pressure of the external air shuts the valve O, and a void is left below the piston: there is therefore no pressure on the upper side of the valve M, to balance the elasticity of the air in the receiver which formerly balanced the weight of the atmosphere. The air, therefore, in the receiver lifts this valve, and distributes itself between the vessel and the barrel; so that when the piston has reached the top, the density of the air in both receiver and barrel is as before $\frac{V}{V+B}$.

When the piston is let go it descends, because the elasticity of the expanded air is not a balance for the pressure of the atmosphere, which therefore presses down the piston with the difference, keeping the piston-valve shut all the while. At the same time the valve M also shuts: for it was opened by the prevailing elasticity of the air in the receiver, and while it is open, the two airs have equal density and elasticity; but the moment the piston descends, the capacity of the barrel is diminished, the elasticity of its air increases by collapsing, and now prevailing over that of the air in the receiver, shuts the valve M.

When it has arrived at such a part of the barrel that the air in it is of the density of the external air, there is no force to push it farther down; the hand must therefore press it. This attempts to condense the air in the barrel, and therefore increases its elasticity; so that it lifts the valve O and escapes, and the piston gets to the bottom. When drawn up again, greater force is required than the last time, because the elasticity of the included air is less than in the former stroke. The piston rises further before the valve M is lifted up, and when it has reached the top of the barrel the density of the included air is $\overline{\frac{\quad}{V+B}}\Big|^{2}$.

The piston, when let go, will descend further than it did before ere the piston-valve open, and the pressure of the hand will again push it to the bottom, all the air escaping through O. The rarefaction will go on at every successive stroke in the same manner as with the other syringe.

This syringe is evidently more easy in its use, requiring no attendance to the cocks to open and shut them at the proper times. On this account this construction of an exhausting syringe is much more generally used.

But it is greatly inferior to the syringe with cocks with respect to its power of rarefaction. Its operation is greatly limited. It is evident that no air will come out of the receiver unless its elasticity *exceed* that of the air in the barrel by a difference able to lift up the valve M. A piece of oiled leather tied across this hole can hardly be made tight and certain of clapping to the hole without some small straining, which must therefore be overcome. It must be very gentle indeed not to require a force equal to the weight of two inches of water, and this is equal to about the 200th part of the whole elasticity of the ordinary air; and therefore this syringe, for this reason alone, cannot rarefy air above 200 times, even though air were capable of an indefinite expansion. In like manner the valve O cannot be raised without a similar prevalence of the elasticity of the air in the barrel above the weight of the atmosphere. These causes united, make it difficult to rarefy the air more than 100 times, and very few such syringes will rarefy it more than 50 times; whereas the syringe with cocks, when new and in good order, will rarefy it 1000 times.

But, on the other hand, syringes with cocks are much more expensive, especially when furnished with apparatus for opening and shutting the cocks. They are more difficult to make equally tight, and (which is the greatest objection) do not remain long in good order. The cocks, by so frequently opening and shutting, grow loose, and allow the air to escape. No method has been found of preventing this. They must be ground tight by means of emery or other cutting powders. Some of these unavoidably stick in the metal, and continue to wear it down. For this reason philosophers, and the makers of philosophical instruments, have turned their chief attention to the improvement of the syringe with valves. We have been thus minute in the account of the operation of rarefaction, that the reader may better understand the value of these improvements, and in general the operation of the principal pneumatic engines.

Of the Air-Pump.

An Air-Pump is nothing but an exhausting syringe, accommodated to a variety of experiments. It was first invented by Otto Guericke, a gentleman of Magdeburg, in Germany, about the year 1654. We trust that it will not be unacceptable to our readers to see this instrument, which now makes a principal article in a philosophical apparatus, in its first form, and to trace it through its successive steps to its present state of improvement.

Guericke, indifferent about the solitary possession of an invention which gave entertainment to numbers who came to see his wonderful experiments, gave a minute description of all his pneumatic apparatus to Gaspar Schottus, professor of mathematics at Wirtemberg, who immediately published it with the author's consent, with an account of some of its performances, first in 1657, in his *Mechanica Hydraulico-pneumatica;* and then in his *Technica Curiosa,* in 1664, a curious collection of all the wonderful performances of art which he collected by a correspondence over all Europe.

Otto Guericke's air-pump consists of a glass receiver A (Fig. 16.), of a form nearly spherical, fitted up with a brass cap and cock B. The nozzle of the cap was fixed to a syringe CDE, also of brass, bent at D into half a right angle. This had a valve at D, opening from the receiver into the syringe, and shutting when pressed in the opposite direction. In the upper side of the syringe there is another valve F, opening from the syringe into the external air, and shutting when pressed inwards. The piston had no valve. The syringe, the cock B, and the joint of the tube, were immersed in a cistern filled with water. From this description it is easy to understand the operation of the instrument. When the piston was drawn up from the bottom of the syringe, the valve F was kept shut by the pressure of the external air, and the valve D opened by the elasticity of the air in the receiver. When it was pushed down again, the valve D immediately shut by the superior elasticity of the air in the syringe; and when this was sufficiently compressed, it opened the valve F, and was discharged. It was immersed in water, that no air might find its way through the joints or cocks.

It would seem that this machine was not very perfect, for Guericke says that it took several hours to produce an evacuation of a moderate-sized vessel; but he says, that when it was in good order, the rarefaction (for he acknowledges that it was not, nor could be, a complete evacuation) was so great, that when the cock was opened, and water admitted, it filled the receiver so as sometimes to leave no more than the bulk of a pea filled with air. This is a little surprising; for if the valve F be placed as far from the bottom of the syringe as in Schottus's figure, it would appear that the rarefaction could not be greater than what must arise from the air in DF expanding till it filled the whole syringe; because as soon as the piston in its descent passes F it can discharge no more air, but must compress it between F and the bottom, to be expanded again when the piston is drawn up. It is probable that the piston was not very tight, but that on pressing it down it allowed the air to pass it; and the water in which the whole was immersed prevented the return of the air when it was drawn up again; and this accounts for the great time necessary for producing the desired rarefaction.

Guericke, being a gentleman of fortune, spared no expense, and added a part to the machine, which saved his numerous visitants the trouble of hours attendance before they could see the curious experiments with the rarefied air. He made a large copper vessel G (Fig. 17.), having a pipe and cook below, which passed through the floor of the chamber into an under apartment, where it was joined to the syringe immersed in the cistern of water, and worked by a lever. The upper part of the vessel terminated in a pipe, furnished with a stopcock H, surrounded with a small brim to hold water for preventing the ingress of air. On the top was another cap I, also filled with water to protect the junction of the pipes with the receiver K. This great vessel was always kept exhausted, and workmen attended below. When experiments were to be performed in the receiver K, it was set on the top of the great vessel, and the cock H was opened. The air in K immediately diffused itself equally between the two vessels, and was so much more rarefied as the receiver K was smaller than the vessel G. When this rarefaction was not sufficient, the attendants below immediately worked the pump.

These particulars deserve to be recorded, as they show the inventive genius of this celebrated philosopher, and because they are useful even in the present advanced state of the study. Guericke's method of excluding air from all the joints of his apparatus, by immersing these joints in water, is the only method that has to this day been found effectual; and there frequently occur experiments where this exclusion for a long time is absolutely necessary. In such cases it is necessary to construct little cups or cisterns at every joint,and to fill them with water or oil. In a letter to Schottus, 1662-3, he describes very ingenious contrivances for producing complete rarefaction after the elasticity of the remaining air has been so far diminished that i is not able to open the valves. He opens the exhausting valves by a plug, which is pushed in by the hand; and the discharging valve is opened by a small pump placed on its outside, so that it opens into a void instead of opening against the pressure of the atmosphere. (See Schotti *Technica Curiosa*, p. 68, 70.) These contrivances have been lately added to air-pumps by Haas and Hurter as new inventions.

It must be acknowledged, that the application of the pump or syringe to the exhaustion of air was a very obvious thought on the principle exhibited in No 17, and in this way it was also employed by Guericke, who first filled the receiver with water, and then applied the syringe. But this was by no means either his object or his principle. His object was not solely to procure a vessel void of air, but to exhaust the air which was already in it; and his principle was the power which he suspected to be in air of expanding itself into a greater space when the force was removed which he supposed to compress it. He expressly says (*Tract. de Experimentis Magdeburgicis, et in Epist. ad Schotum*), that the contrivance occurred to him accidentally when occupied with experiments on the Toricellian tube, in which he found that the air would really expand, and completely fill a much larger space than what it usually occupied, and that he had found no limits to the expansion, evincing this by facts which we shall perfectly understand by and by. This was a doctrine quite new, and required a philosophical mind to view it in a general and systematic manner; and it must be owned that his manner of treating the subject is equally remarkable for ingenuity and for modesty. (*Epist. ad Schottum.*)

His doctrine and his machine were soon spread over Europe. It was the age of literary ardour and philosophical curiosity; and it is most pleasing to us, who, standing on the shoulders of our predecessors, can see far around us, to observe the eagerness with which every new, and to us frivolous, experiment was repeated and canvassed. The worshippers of Aristotle were daily receiving severe mortifications from the experimenters, or empirics, as they affected to call them, and they exerted themselves strenuously in support of his now tottering cause. This contributed to the rapid propagation of every discovery; and it was a most profitable and respectable business to go through the chief cities of Germany and France exhibiting philosophical experiments.

About this time the foundations of the Royal Society of London were laid. Mr Boyle, Mr Wren, Lord Brounker, Dr Wallis, and other curious gentlemen, held meetings at Oxford, in which were received accounts of whatever was doing in the study of nature; and many experiments were exhibited. The researches of Galileo, Toricelli, and Paschal, concerning the pressure of the air, greatly engaged their attention, and many additions were made to their discoveries. Mr Boyle, the most ardent and successful studier of nature, had the principal share in these improvements, his inquisitive mind being aided by an opulent fortune. In a letter to his nephew, Lord Dungarvon, he says that he had made many attempts to see the appearances exhibited by bodies freed from the pressure of the air. He had made Toricellian tubes, having a small vessel a-top, into which he put some bodies before filling the tubes with mercury; so that when the tube was set upright, and the mercury run out, the bodies were *in vacuo*. He had also abstracted the water from a vessel, by a small pump, by means of its weight, in the manner described in No 17, having previously put bodies into the vessel along with the water. But all these ways were very troublesome and imperfect. He was delighted when he learned from Schottus's first publication, that Counsellor Guericke had effected this by the expansive power of the air; and immediately set about constructing a machine from his own ideas, no description of Guericke's being then published.

It consisted of a receiver A (Fig. 18.), furnished with a stopcock B, and syringe CD placed in a vertical position below the receiver. Its valve C was in its bottom, close adjoining to the entry of the pipe of communication; and the hole by which the air issued was farther secured by a plug which could be removed. The piston was moved by a wheel and rackwork. The receiver of Guericke's pump was but ill adapted for any considerable variety of experiments; and accordingly very few were made in it. Mr Boyle's receiver had a large opening EF, with a strong glass margin. To this was fitted a strong brass cap, pierced with a hole G in its middle, to which was fitted a plug ground into it, and shaped like the key of a cock. The extremity of this key was furnished with a screw, to which could be affixed a hook, or a variety of pieces for supporting what was to be examined in the receiver, or for producing various motions within it, without admitting the air. This was farther guarded against by means of oil poured round the key, where it was retained by the hollow cup-like form of the cover. With all these precautions, however, Mr Boyle ingeniously confesses, that it was but seldom, and with great difficulty, that he could produce an extreme degree of rarefaction; and it appears by Guericke's letter to Schottus, that in this respect the Magdeburgh machine had the advantage. But most of Boyle's very interesting experiments did not require this extreme rarefaction; and the variety of them, and their philosophic importance, compensated for this defect, and soon eclipsed the fame of the inventor to such a degree, that the state of air in the receiver was generally denominated the *vacuum Boyleanum*, and the air-pump was called *machina Boyleana*. It does not appear that Guericke was at all solicitous to maintain his claim to priority or invention. He appears to have been of a truly noble and philosophical mind, aiming at nothing but the advancement of science.

Mr Boyle found, that to make a vessel air-tight, it was sufficient to place a piece of wet or oiled leather on its brim, and to lay a flat plate of metal upon this. The pressure of the extenal air squeezed the two solid bodies so hard together, that the soft leather effectually excluded it. This enabled him to render the whole machine incomparably more convenient for a variety of experiments. He caused the conduit-pipe to terminate in a flat plate which he covered with leather, and on this he set the glass ball or receiver, which had both its upper and lower brim ground flat. He covered the upper orifice in like manner with a piece of oiled leather and a flat plate, having cocks and a variety of other perforations and contrivances suited to his purposes. This he found infinitely more expeditious, and also tighter, than the clammy cements which he had formerly used for securing the joints.

He was now assisted by Dr Hooke, the most ingenious and inventive mechanic that the world has ever seen. This person made a great improvement on the air-pump, by applying two syringes whose piston-rods were worked by the same wheel, as in Fig. 20, and putting valves in the pistons in the same manner as in the piston of a common pump. This evidently doubled the expedition of the pump's operation: but it also greatly diminished the labour of pumping; for it must be observed, that the piston H must be drawn up against the pressure of the external air, and when the rarefaction is nearly perfect, this requires a force of nearly 15 pounds for every inch of the area of the piston. Now when one piston H is at the bottom of the barrel, the other K is at the top of the barrel, and the air below K is equally rare with that in the receiver. Therefore the pressure of the external air on the piston K is *nearly* equal to that on the piston H. Both, therefore, are acting in opposite directions on the wheel which gave them motion; and the force necessary for raising H is only the difference between the elasticity of the air in the barrel H and that of the air in the barrel K. This is very small in the beginning of the stroke, but gradually increases as the piston K descends, and becomes equal to the whole excess of the air's pressure above the elasticity of the remaining air of the receiver when the air at K of the natural density begins to open the piston valves. An accurate attention to the circumstances will show us that the force requisite for working the pump is greatest at first, and gradually diminishes as the rarefaction advances; and when this is nearly complete, hardly any more force is required than what is necessary for overcoming the friction of the pistons, except during the discharge of the air at the end of each stroke.

This is therefore the form of the air-pump which is most generally used all over Europe. Some traces of national prepossession remain. In Germany, air-pumps are frequently made after the original model of Guericke's (Wolff Cyclomathesis); and the French generally use the pump made by Papin, though extremely awkward. We shall give a description of Boyle's air-pump as finally improved by Hawkesbee, which, with some small accommodations to particular views, still remains the most approved form.

Here follows the description from Desaguliers:

It consists of two brass barrels *a a*, *a a* (fig. 19.), 12 inches high and 2 wide. The pistons are raised and depressed by turning the winch *b b*. This is fastened to an axis passing through a strong-toothed wheel, which lays hold of the teeth of the racks *c c c c*. Then the one is raised while the other is depressed; by which means the valves, which are made of limber bladder, fixed in the upper part of each piston, as well as in the openings into the bottom of the barrels, perform their office of discharging the air from the barrels, and admitting into them the air from the receiver to be afterwards discharged; and when the receiver comes to be pretty well exhausted of its air, the pressure of the atmosphere in the descending piston is nearly so great, that the power required to raise the other is little more than is necessary for overcoming the friction of the piston, which renders this pump preferable to all others, which require more force to work them as the rarefaction of the air in the receiver advances.

The barrels are set in a brass dish about two inches deep, filled with water or oil to prevent the insinuation of air. The barrels are screwed tight down by the nuts *e e*, *e e*, which force the frontispiece *f f* down on them, through which the two pillars *g g*, *g g* pass.

From between the barrels rises a slender brass pipe *h h*, communicating with each by a perforation in the transverse piece of brass on which they stand. The upper end of this pipe communicates with another perforated piece of brass, which screws on underneath the plate *i i i i*, of ten inches diameter, and surrounded with a brass rim to prevent the shedding of water used in some experiments. This piece of brass has three branches: 1. An horizontal one communicating with the conduit-pipe *h h*. 2. An upright one screwed into the middle of the pump-plate, and terminating in a small pipe *k*, rising about an inch above it. 3. Is a perpendicular one, looking downwards in the continuation of the pipe *k*, and having a hollow screw in its end receiving the brass cap of the gage-pipe *l l l l*, which is of glass, 34 inches long, and immersed in a glass cistern *m m* filled with mercury. This is covered a-top with a cork float, carrying the weight of a light wooden scale divided into inches, which are numbered from the surface of the mercury in the cistern. This scale will therefore rise and fall with the mercury in the cistern, and indicate the true elevation of that in the tube.

There is a stopcock immediately above the insertion of the gage-pipe, by which its communication may be cut off. There is another at *n*, by which a communication is opened with the external air for allowing its readmission; and there is sometimes another immediately within the insertion of the conduct-pipe for cutting off the communication between the receiver and the pump. This is particularly useful when the rarefaction is to be continued long, as there are by these means fewer chances of the insinuation of air by the many joints.

The receivers are made tight by simply setting them on the pump-plate with a piece of wet or oiled leather between; and the receivers, which are open a-top, have a brass cover set on them in the same manner. In these covers there are various perforations and contrivances for various purposes. The one in the figure has a slip wire passing through a collar of oiled leather, having a hook or a screw in its lower end for hanging any thing on or producing a variety of motions.

Sometimes the receivers are set on another plate, which has a pipe screwed into its middle, furnished with a stopcock and a screw, which fits the middle pipe *k*. When the rarefaction has been made in it, the cock is shut, and then the whole may be unscrewed from the pump, and removed to any convenient place. This is called a *transporter plate*.

It only remains to explain the gage *l l l l*. In the ordinary state of the air its elasticity balances the pressure of the incumbent atmosphere. We find this from the force that is necessary to squeeze it into less bulk in opposition to this elasticity. Therefore the elasticity of the air increases with the vicinity of its particles. It is therefore reasonable to expect, that when we allow it to occupy more room, and its particles are farther asunder, its elasticity will be diminished though not annihilated; that is, it will no longer balance the WHOLE pressure of the atmosphere, though it may still balance part of it. If therefore an upright pipe have its lower end immersed in a vessel of mercury, and communicate by its upper end with a vessel containing rarefied, therefore less elastic, air, we should expect that the pressure of the air will prevail, and force the mercury into the tube, and cause it to rise to such an height that the weight of the mercury, joined to the elasticity of the rarefied air acting on its upper surface, shall be exactly equal to the whole pressure of the atmosphere. The height of the mercury is the exact measure of that part of the whole pressure which is not balanced by the elasticity of the rarefied air, and its deficiency from the height of the mercury in the Toricellian tube is the exact measure of this remaining elasticity.

It is evident therefore, that the pipe will be a scale of the elasticity of the remaining air, and will indicate in some sort the degree of rarefaction: for there must be some analogy between the density of the air and its elasticity; and we have no reason to imagine that they do not increase and diminish together, although we may be ignorant of the law, that is, of the change of elasticity corresponding to a known change of density. This is to be discovered by experiment; and the air-pump itself furnishes us with the best experiments for this purpose. After rarefying till the mercury in the gage has attained half the height of that in the Toricellian tube, shut the communication with the barrels and gage, and admit the water into the receiver. It will go in till all is again in equilibrio with the pressure of the atmosphere; that is, till the air in the receiver has collapsed into its natural bulk. This we can accurately measure, and compare with the whole capacity of the receiver; and thus obtain the precise degree of rarefaction corresponding to half the natural elasticity. We can do the same thing with the elasticity reduced to one-third, one-fourth, &c. and thus discover the whole law.

This gage must be considered as one of the most ingenious and convenient parts of Hawkesbee's pump; and it is well disposed, being in a situation protected against accidents; but it necessarily increases greatly the size of the machine, and cannot be applied to the table-pump, represented in Fig. 20, No. 1. When it is wanted here, a small plate is added behind, or between the barrels and receiver; and on this is set a small tubulated (as it is termed) receiver, covering a common weather-glass tube. This receiver being rarefied along with the other, the pressure on the mercury in the cistern, arising from the elasticity of the remaining air, is diminished so as to be no longer able to support the mercury at its full height; and it therefore descends till the height at which it stands puts it in equilibrio with the elasticity. In this form, therefore, the height of the mercury is directly a measure of the remaining elasticity; while in the other it measures the remaining unbalanced pressure of the atmosphere. But this gage is extremely cumbersome and liable to accidents. We are seldom much interested in the rarefaction till it is great: a contracted form of this gage is therefore very useful, and was early used. A syphon ABCD (Fig. 21.) each branch of which is about four inches long, and close at A and open at D, is filled with boiling mercury till it occupies the branch AB and a very small part of CD, having its surface at O. This is fixed to a small stand, and fixed into the receiver, along with the things that are to be exhibited in the rarefied air. When the air has been rarefied till its remaining elasticity is not able to support the column BA, the mercury descends in AB, and rises in CD, and the remaining elasticity will always be measured by the elevation of the mercury in AB above that in the leg CD. Could the exhaustion be perfected, the surfaces in both legs would be on a level. Another gage might be put into the same foot, having a small bubble of air at A. This would move from the beginning of the rarefaction; but our ignorance of the analogy between the density and elasticity hinders us from using it as a measure of either.

It is enough for our present purpose to observe, that the barometer or syphon gage is a perfect indication and measure of the performance of an air-pump, and that a pump is (*cæteris paribus*) so much the more perfect, as it is able to raise the mercury higher in the gage. It is in this way that we discover that none can produce a complete exhaustion, and that their operation is only a very great rarefaction; for none can raise the mercury to that height at which it stands in the Toricellian tube, well purged of air. Few pumps will bring it within $\frac{1}{10}$ of an inch. Hawksbee's, fitted up according to his instructions, will seldom bring it within $\frac{1}{3}$. Pumps with cocks, when constructed according to the principles mentioned when speaking of the exhausting syringe, and new and in fine order, will in favourable circumstances bring it within $\frac{1}{20}$. None with valves, fitted up with wet leather, or when water or volatile fluids are allowed access into any part, will bring it nearer than $\frac{1}{5}$. Nay, a pump of the best kind, and in the finest order, will have its rarefying power reduced to the lowest standard, as measured by this gage, if we put into the receiver the tenth part of a square inch of white sheep-skin, fresh from the shops, or of any substance equally damp. This is a discovery made by means of the improved air-pump, and leads to very extensive and important consequences in general physics; some of which will be treated of under this article: and the observation is made thus early, that our readers may better understand the improvements which have been made on this celebrated machine.

It would require a volume to describe all the changes which have been made on it. An instrument of such multifarious use, and in the hands of curious men, each diving into the secrets of nature in his favourite line, must have received many alterations and real improvements in many particular respects. But these are beside our present purpose; which is to consider it merely as a machine for rarefying elastic or expansive fluids. We must therefore confine ourselves to this view of it; and shall carefully state to our readers every improvement founded on principle, and on pneumatical laws.

All who used it perceived the limit set to the rarefaction by the resistance of the valves, and tried to perfect the construction of the cocks. The Abbé Nollet and Gravesande, two of the most eminent experimental philosophers in Europe, were the most successful.

Mr Gravesande justly preferred Hooke's plan of a double pump, and contrived an apparatus for turning the cocks by the motion of the pump's handle. This is far from either being simple or easy in working; and occasions great jerks and concussions in the whole machine. This, however, is not necessarily connected with the truly pneumatical improvement. His piston has no valve, and the rod is connected with it by a stirrup D (Fig. 22.), as in a common pump. The rod has a cylindric part, *c p*, which passes through the stirrup, and has a stiff motion in it up and down of about half an inch; being stopped by the shoulder *c* above and the nut below. The round plate supported by this stirrup has a short square tube *n d*, which fits tight into the hole of a piece of cork F. The round plate E has a square shank *g*, which goes into the square tube *n d*. A piece of thin leather *f*, soaked in oil, is put between the cork and the plate E, and another between the cork and the plate which forms the sole of the stirrup. All these pieces are screwed together by the nail *e*, whose flat head covers the hole *n*. Suppose, therefore, the piston touching the bottom of the barrel, and the winch turning to raise it again, the friction of the piston on the barrel keeps it in its place, and the rod is drawn up through the stirrup D. Thus the wheel has liberty to turn about an inch; and this is sufficient for turning the cock, so as to cut off the communication with the external air, and to open the communication with the receiver. This being done, and the motion of the winch continued, the piston is raised to the top of the barrel. When the winch is turned in the opposite direction, the piston remains fixed till the cock is turned, so as to shut the communication with the receiver, and open that with the external air.

This is a pretty contrivance, and does not at first appear necessary; because the cocks might be made to turn at the beginning and end of the stroke without it. But this is just possible; and the smallest error of adjustment, or wearing of the apparatus, will cause them to be open at improper times. Besides, the cocks are not turned in an instant, and are improperly open during some very small time; but this contrivance completely obviates this difficulty.

The cock is precisely similar to that formerly described, having one perforation diametrically through it, and another entering at right angles to this, and after reaching the centre, it passes along the axis of the cock, and comes out to the open air.

It is evident, that by this construction of the cock, the ingenious improvement of Dr Hooke, by which the pressure of the atmosphere on one piston is made to balance (in great part) the pressure on the other, is given up: for, whenever the communication with the air is opened, it rushes in, and immediately balances the pressure on the upper side of the piston in this barrel; so that the whole pressure in the other must be overcome by the person working the pump. Gravesande, aware of this, put a valve on the orifice of the cock; that is, tied a slip of wet bladder or oiled leather across it; and now the piston is pressed down, as long as the air in the barrel is rarer than the outward air, in the same manner as when the valve is in the piston itself.

This is all that is necessary to be described in Mr Gravesande's air-pump. Its performance is highly extolled by him, as far exceeding his former pumps with valves. The same preference was given to it by his successor Muschenbroek. But, while they both prepared the pistons and valves and leathers of the pump, by steeping them in oil, and then in a mixture of water and spirits of wine, we are certain that no just estimate could be made of its performance. For with this preparation it could not bring the gage within ½ of an inch of the barometer. We even see other limits to its rarefaction: from its construction, it is plain that a very considerable space is left between the piston and cock, not less than an inch, from which the air is never expelled; and if this be made extremely small, it is plain that the pump must be worked very slow, otherwise there will not be time for the air to diffuse itself from the receiver into the barrel, especially towards the end, when the expelling force, viz. the elasticity of the remaining air is very small. There is also the same limit to the rarefaction, as in Hooke's or Hawkesbee's pump, opposed by the valve E, which will not open till the air below the piston is considerably denser than the external air: and this pump soon lost any advantages it possessed when fresh from the workman's hands, by the cock's growing loose and admitting air. It is surprising that Gravesande omitted Hawkesbee's security against this, by placing the barrels in a dish filled with oil; which would effectually have prevented this inconvenience.

We must not omit a seemingly paradoxical observation of Gravesande, that in a pump constructed with valves, and worked with a determined uniform velocity, the required degree of rarefaction is sooner produced by short barrels than by long ones. It would require too much time to give a general demonstration of this, but it will easily be seen by an example. Suppose the long barrel to have equal capacity with the receiver, then at the end of the first stroke the air in the receiver will have $\frac{1}{2}$ its natural density. Now, let the short barrels have half this capacity; at the end of the first stroke the density of the air in the receiver is $\frac{2}{3}$, and at the end of the second stroke it is $\frac{4}{9}$, which is less than $\frac{1}{2}$, and the two strokes of the short barrel are supposed to be made in the same time with one of the longest, &c.

Hawkesbee's pump maintained its pre-eminence without rival in Britain, and generally too on the continent, except in France, where every thing took the *ton* of the Academy, which abhorred being indebted to foreigners for any thing in science, till about the year 1750, when it engaged the attention of Mr John Smeaton, a person of uncommon knowledge, and second to none but Dr Hooke in sagacity and mechanical resource. He was then a maker of philosophical instruments, and made many attempts to perfect the pumps with cocks, but found, that whatever perfection he could bring them to, he could not enable them to preserve it; and he never would sell one of this construction. He therefore attached himself solely to the valve pumps.

The first thing was to diminish the resistance to the entry of the air from the receiver into the barrels: this he rendered almost nothing, by enlarging the surface on which this feeble elastic air was to press. Instead of making these valves to open by its pressure on a circle of $\frac{1}{20}$ of an inch in diameter, he made the valve-hole one inch in diameter, enlarging the surface 400 times; and to prevent this piece of thin leather from being burst by the great pressure on it, when the piston in its descent was approaching the bottom of the barrel, he supported it by a delicate but strong grating, dividing the valve-hole like the section of a honey-comb, as represented in Fig. 25; and the ribs of this grating are seen edgewise in Fig. 23, at *a b c*.

The valve was a piece of thin membrane or oiled silk, gently strained over the mouth of the valve-hole, and tied on by a fine silk thread wound round it in the same manner that the narrow slips had been tied on formerly. This done, he cut with a pointed knife the leather round the edge, nearly four quadrantal arcs, leaving a small tongue between each, as in Fig. 25. The strained valve immediately shrinks inwards, as represented by the shaded parts; and the strain by which it is kept down is now greatly diminished, taking place only at the corners. The gratings being reduced nearly to an edge (but not quite, lest they should cut), there is very little pressure to produce adhesion by the clammy oil. Thus it appears, that a very small elasticity of the air in the receiver will be sufficient to raise the valve; and Mr Smeaton found, that when it was not able to do this at first, when only about $\frac{1}{600}$ of the natural elasticity, it would do it after keeping the piston up eight or ten seconds, the air having been all the while undermining the valve, and gradually detaching it from the grating.

Unfortunately he could not follow this method with the piston valve. There was not room round the rod for such an expanded valve; and it would have obliged him to have a great space below the valve, from which he could not expel the air by the descent of the piston. His ingenuity hit on a way of increasing the expelling force through the common valve: he enclosed the rod of the piston in a collar of leather *l*, through which it moved freely without allowing any air to get past its sides. For greater security, the collar of leather was contained in a box terminating in a cup filled with oil. As this makes a material change in the principle of construction of the air-pump (and indeed of pneumatic engines in general), and as it has been adopted in all the subsequent attempts to improve them, it merits a particular consideration.

The piston itself consists of two pieces of brass fastened by screws from below. The uppermost, which is of one solid piece with the rod GH (Fig. 23.), is of a diameter somewhat less than the barrel; so that when they are screwed together, a piece of leather, soaked in a mixture of boiled oil and tallow, is put between them; and when the piston is thrust into the barrel from above, the leather comes up around the side of the piston, and fills the barrel, making the piston perfectly air-tight. The lower half of the piston projects upwards into the upper, which has a hollow *g b c g* to receive it. There is a small hole through the lower half at *a* to admit the air; and a hole *c d* in the upper half to let it through, and there is a slip of oiled silk strained across the hole *a* by way of valve, and there is room enough left at *b c* for this valve to rise a little when pressed from below. The rod GH passes through the piece of brass which forms the top of the barrel so as to move freely, but without any sensible shake: this top is formed into a hollow box, consisting of two pieces ECDF and CNOD, which screw together at CD. This box is filled with rings of oiled leather exactly fitted to its diameter, each having a hole in it for the rod to pass through. When the piece ECDF is screwed down, it compresses the leathers; squeezing them to the rod, so that no air can pass between them; and, to secure us against all ingress of air, the upper part is formed into a cup EF, which is kept filled with oil.

The top of the barrel is also pierced with a hole LK, which rises above the flat surface NO, and has a slip of oiled silk tied over it to act as a valve; opening when pressed from below, but shutting when pressed from above.

The communication between the barrel and receiver is made by means of the pipe ABPQ; and there goes from the hole K in the top of the barrel a pipe KRST, which either communicates with the open air or with the receiver, by means of the cock at its extremity T. The conduit pipe ABPQ has also a cock at Q, by which it is made to communicate either with the receiver or with the open air. These channels of communication are variously conducted and terminated, according to the views of the maker: the sketch in this figure is sufficient for explaining the principle, and is suited to the general form of the pump, as it has been frequently made by Nairne and other artists in London.

Let us now suppose the piston at the top of the barrel, and that it applies to it all over, and that the air in the barrel is very much rarefied: in the common pump the piston valve is pressed hard down by the atmosphere, and continues shut till the piston gets far down, condenses the air below it beyond its natural state, and enables it to force up the valves. But here, as soon as the piston quits the top of the barrel, it leaves a void behind it; for no air gets in round the piston rod, and the valve at K is shut by the pressure of the atmosphere. There is nothing now to oppose the elasticity of the air below but the stiffness of the valve *b c*; and thus the expelling (or more accurately the liberating) force is prodigiously increased.

The superiority of this construction will be best seen by an example. Suppose the stiffness of the valve equal to the weight of $\frac{1}{10}$ of an inch of mercury, when the barometer stands at 30 inches, and that the pump gage stands at 29.9; then, in an ordinary pump, the valve in the piston will not rise till the piston has got within the 300th part of the bottom of the barrel, and it will leave the valve-hole filled with air of the ordinary density. But in this pump the valve will rise as soon as the piston quits the top of the barrel; and when it is quite down, the valve-hole *a* will contain only the 300th part of the air which it would have contained in a pump of the ordinary form. Suppose further, that the barrel is of equal capacity with the receiver, and that both pumps are so badly constructed, that the space left below the piston is the 300th part of the barrel. In the common pump the piston valve will rise no more, and the rarefaction can be carried no farther, however delicate the barrel valve may be; but in this pump the next stroke will raise the gage to 29.95, and the piston valve will again rise as soon as the piston gets half way down the barrel.

The limit to the rarefaction by this pump depends chiefly on the space contained in the hole LK; and in the space *b c d* of the piston. When the piston is brought up to the top, and applied close to it, those spaces remain filled with the air of the ordinary density, which will expand as the piston descends, and thus will retard the opening of the piston valve. The rarefaction will stop when the elasticity of this small quantity of air, expanded so as to fill the whole barrel (by the descent of the piston to the bottom), is just equal to the force requisite for opening the piston valve.

Another advantage attending this construction is, that in drawing up the piston, we are not resisted by the whole pressure of the air; because the air is rarefied above this piston as well as below it, and the piston is in precisely the same state of pressure as if connected with another piston in a double pump. The resistance to the ascent of the piston is the excess of the elasticity of the air above it over the elasticity of the air below: this, toward the end of the rarefaction, is very small, while the piston is near the bottom of the barrel, but gradually increases as the piston rises, and reduces the air above it into smaller dimensions, and becomes equal to the pressure of the atmosphere, when the air above the piston is of the common density. If we should raise the piston still farther, we must condense the air above it: but Mr Smeaton has here made an issue for the air by a small hole in the top of the barrel, covered with a delicate valve. This allows the air to escape, and shuts again as soon as the piston begins to descend, leaving almost a perfect void behind it as before.

This pump has another advantage. It may be changed in a moment from a rarefying to a condensing engine, by simply turning the cocks at Q and T. While T communicates with the open air and Q with the receiver, it is a rarefying engine or air-pump: but when T communicates with the receiver, and Q with the open air, it is a condensing engine.

Fig. 26. represents Mr Smeaton's air-pump as it is usually made by Nairne. Upon a solid base or table are set up three pillars F, H, H: the pillar F supports the pump-plate A; and the pillars H, H, support the front or head, containing a brass cog-wheel, which is turned by the handle B, and works in the rack C fastened to the upper end of the piston rod. The whole is still farther steadied by two pieces of brass *c b* and *o k*, which connect the pump-plate with the front, and have perforations communicating between the hole *a* in the middle of the plate and the barrel, as will be described immediately. DE is the barrel of the pump, firmly fixed to the table by screws through its upper flanch: *a f d e* is a slender brass tube screwed to the bottom of the barrel, and to the under hole of the horizontal canal *c b*. In this canal there is a cock which opens a communication between the barrel and the receiver, when the key is in the position represented here: but when the key is at right angles with this position, this communication is cut off. If that side of the key which is here drawn next to the pump-plate be turned outward, the external air is admitted into the receiver; but if turned inwards, the air is admitted into the barrel.

g h is another slender brass-pipe, leading from the discharging valve at *g* to the horizontal canal *h k*, to the under side of which it is screwed fast. In this horizontal canal there is a cock *n* which opens a passage from the barrel to the receiver when the key is in the position here drawn; but opens a passage from the barrel to the external air when the key is turned outwards, and from the receiver to the external air when the key is turned inwards. This communication with the external air is not immediate, but through a sort of box *i*; the use of this box is to receive the oil which is discharged through the top valve *g*. In order to keep the pump tight, and in working order, it is proper sometimes to pour a table-spoonful of olive-oil into the hole *a* of the pump-plate, and then to work the pump. The oil goes along the conduit *b c d f e*, gets into the barrel and through the piston-valve, when the piston is pressed to the bottom of the barrel, and is then drawn up, and forced through the discharging valve *g* along the pipe *g h*, the horizontal passage *h n*, and finally into the box *i*. This box has a small hole in its side near the top, through which the air escapes.

From the upper side of the canal *cb* there rises a slender pipe which bends outward and then turns downwards, and is joined to a small box, which cannot be seen in this view. From the bottom of this box proceeds downwards the gage-pipe of glass, which enters the cistern of mercury G fixed below.

On the upper side of the other canal at *o* is seen a small stud, having a short pipe of glass projecting horizontally from it, close by and parallel to the front piece of the pump, and reaching to the other canal. This pipe is close at the farther end, and has a small drop of mercury or oil in at the end *o*. This serves as a gage in condensing, indicating the degree of condensation by the place of the drop: for this drop is forced along the pipe, condensing the air before it in the same degree that it is condensed in the barrel and receiver.

In constructing this pump, Mr Smeaton introduced a method of joining together the different pipes and other pieces, which has great advantages over the usual manner of screwing them together with leather between, and which is now much used in hydraulic and pneumatic engines. We shall explain this to our readers by a description of the manner in which the exhausting gage is joined to the horizontal duct *c b*.

The piece *h i p*, in Fig. 24. is the same with the little cylinder observable on the upper side of the horizontal canal *c d*, in Fig. 26. The upper part *h i* is formed into an outside screw, to fit the hollow screw of the piece *d e e d*. The top of this last piece has a hole in its middle, giving an easy passage to the bent tube *c b a*, so as to slip along it with freedom. To the end *c* of this bent tube is soldered a piece of brass *c f g*, perforated in continuation of the tube, and having its end ground flat on the top of the piece *h i p*, and also covered with a slip of thin leather strained across it and pierced with a hole in the middle.

It is plain from this form, that if the surface *f g* be applied to the top of *h i*, and the cover *d e e d* be screwed down on it, it will draw or press them together, so that no air can escape by the joint, and this without turning the whole tube *c b a* round, as is necessary in the usual way. This method is now adopted for joining together the conducting pipes of the machines for extinguishing fires,—an operation which was extremely troublesome before this improvement.

The conduit pipe E *e f c* (Fig. 26.) is fastened to the bottom of the barrel, and the discharging pipe *g h* to its top, in the same manner. But to return to the gage: the bent pipe *c b a* enters the box *s t* near one side, and obliquely, and the gage-pipe *q r* is inserted through its bottom towards the opposite side. The use of this box is to catch any drops of mercury which may sometimes be dashed up through the gage-pipe by an accidental oscillation. This, by going through the passages of the pump, would corrode them, and would act particularly on the joints, which are generally soldered with tin. When this happens to an air-pump, it must be cleaned with the most scrupulous attention, otherwise it will be quickly destroyed.

This account of Smeaton's pump is sufficient for enabling the reader to understand its operation and to see its superiority. It is reckoned a very fine pump of the ordinary construction, which will rarefy 200 times, or raise the gage to 29.85, the barometer standing at 30. But Mr Smeaton found, that his pump, even after long using, raised it to 29.95, which we consider as equivalent to rarefying 600 times. When in fine order, he found no bounds to its rarefaction, frequently raising the gage as high as the barometer; and he thought its performance so perfect, that the barometer-gage was not sufficiently delicate for measuring the rarefaction. He therefore substituted the syphon-gage already described, which he gives some reasons for preferring; but even this he found not sufficiently sensible.

He contrived another, which could be carried to any degree of sensibility. It consisted of a glass body A (Fig. 27.), of a pear shape, and was therefore called the pear-gage. This had a small projecting orifice at B, and at the other end a tube CD, whose capacity was the hundredth part of the capacity of the whole vessel. This was suspended at the slip-wire of the receiver, and there was set below it a small cup with mercury. When the pump was worked, the air in the pear-gage was rarefied along with the rest. When the rarefaction was brought to the degree intended, the gage was let down till B reached the bottom of the mercury. The external air being now let in, the mercury was raised into the pear, and stood at some height E in the tube CD. The length of this tube being divided into 100 parts, and those numbered from D, it is evident that $\frac{\text{DE}}{\text{DB}}$ will express the degree of rarefaction which had been produced when the gage was immersed into the mercury; or if DC be $\frac{1}{100}$ of the whole capacity, and be divided into 100 parts by a scale annexed to it, each unit of the scale will be $\frac{1}{10000}$ of the whole.

This was a very ingenious contrivance, and has been the means of making some very curious and important discoveries which at present engage the attention of philosophers. By this gage Mr Smeaton found, that his pump frequently rarefied a thousand, ten thousand, nay an hundred thousand times. But though he in every instance saw the great superiority of his pump above all others, he frequently found irregularities which he could not explain, and a want of correspondence between the pear and the barometer gages which puzzled him. The pear-gage frequently indicated a prodigious rarefaction, when the barometer-gage would not shew more than 600.

These unaccountable phenomena excited the curiosity of philosophers, who by this time were making continual use of the air-pump in their meteorological researches, and much interested in every thing connected with the state or constitution of elastic fluids. Mr Nairne, a most ingenious and accurate maker of philosophical instruments, made many curious experiments in the examination and comparison of Mr Smeaton's pump with those of the usual construction, attending to every circumstance which could contribute to the inferiority of the common pumps or to their improvement, so as to bring them nearer to this rival machine. This rigorous comparison brought into view several circumstances in the constitution of the atmospheric air, and its relation to other bodies, which are of the most extensive and important influence in the operations of nature. We shall notice at present such only as have a relation to the operation of the air-pump in extracting AIR from the receiver.

Mr Nairne found, that when a little water, or even a bit of paper damped with water, was exposed under the receiver of Mr Smeaton's air-pump, when in the most perfect condition, raising the mercury in the barometer-gage to 29.95, he could not make it rise above 29.8 if Fahrenheit's thermometer indicated the temperature 47°, nor above 29.7 if the thermometer stood at 55°; and that to bring the gage to this height and keep it there, the operation of the pump must be continued for a long time after the water had disappeared, or the paper become perfectly dry.

He found that a drop of spirits, or paper moistened with spirits, could not in those circumstances allow the mercury in the gage to rise to near that height; and that similar effects followed from admitting any volatile body whatever into the receiver or any part of the apparatus.

This showed him at once how improper the directions were which had been given by Gueriche, Boyle, Gravesande, and others, for fitting up the air-pump for experiment, by soaking the leather in water, covering the joints with water, or, in short, admitting water or any other volatile body near it.

He therefore took his pumps to pieces, cleared them of all the moisture which he could drive from them by heat, and then leathered them anew with leather soaked in a mixture of olive-oil and tallow, from which he had expelled all the water it usually contains, by boiling it till the first frothing was over. When the pumps were fitted up in this manner, he uniformly found that Mr Smeaton's pump rarefied the gage to 29.95, and the best common pump to 29.87, the first of which he computed to indicate a rarefaction to 600, and the other to 230. But in this state he again found that a piece of damp paper, leather, wood, &c. in the receiver, reduced the performance in the same manner as before.

But the most remarkable phenomenon was, that when he made use of the pear-gage with the pump cleared from all moisture, it indicated the same degree of rarefaction with the barometer-gage: but when he exposed a bit of paper moistened with spirits, and thus reduced the rarefaction of the pump to what he called 50, the barometer-gage standing at 29.4, the pear-gage indicated a rarefaction exceeding 100,000; in short, it was not measurable; and this phenomenon was almost constant. Whenever he exposed any substance susceptible of evaporation, he found the rarefaction indicated by the barometer-gage greatly reduced, while that indicated by the pear-gage was prodigiously increased; and both these effects were more remarkable as the subject was of easier evaporation, or the temperament of the air of the chamber was warmer.

This uniform result suggested the true cause. Water boils at the temperature 212, that is, it is then converted into a vapour which is permanently elastic while of that temperature, and its elasticity balances the pressure of the atmosphere. If this pressure be diminished by rarefying the air above it, a lower temperature will now allow it to be converted into elastic vapour, and keep it in that state. Water will boil in the receiver of an air-pump at the tempermanent 96, or even under it. Philosophers did not think of examining the state of the vapour in temperatures lower than what produced ebullition. But it now appears, that in much lower heats than this the superficial water is converted into elastic vapour, which continues to exhale from it as long as the water lasts, and, supplying the place of air in the receiver, exerts the same elasticity, and hinders the mercury from rising in the gage in the same manner as so much air of equal elasticity would have done.

When Mr Nairne was exhibiting these experiments to the Honourable Henry Cavendish in 1776, this gentleman informed him that it appeared from a series of experiments of his father, Lord Charles Cavendish, that when water is of the temperature 72°, it is converted into vapour, under any pressure less than three-fourths of an inch of mercury, and at 41° it becomes vapour when the pressure is less than one-fourth of an inch: even mercury evaporates in this manner when all pressure is removed. A dewy appearance is frequently observed covering the inside of the tube of a barometer, where we usually suppose a vacuum. This dew, when viewed through a microscope, appears to be a set of detached globules of mercury, and upon inclining the tube so that the mercury may ascend along it, these globules will be all licked up, and the tube become clear. The dew which lined it was the vapour of the mercury condensed by the side of the tube; and it is never observed but when one side is exposed to a stream of cold air from a window, &c.

To return to the vapour in the air-pump receiver, it must be observed, that as long as the water continues to yield it, we may continue to work the pump; and it will be continually abstracted by the barrels, and discharged in the form of water, because it collapses as soon as exposed to the external pressure. All this while the gage will not indicate any more rarefaction, because the thing immediately indicated by the barometer-gage is *diminished elasticity*, which does not happen here. When all the water which the temperature of the room can keep elastic has evaporated under a certain pressure, suppose ½ an inch of mercury, the gage standing at 29.5, the vapour which now fills the receiver expands, and by its diminished elasticity the gage rises, and now some more water, which had been attached to bodies by chemical or corpuscular attraction, is detached, and a new supply continues to support the gage at a greater height; and this goes on continually till *almost* all has been abstracted: but there will remain some which no art can take away; for as it passes through the barrels, and gets between the piston and the top, it successively collapses into water during the ascent of the piston, and again expands into vapour when we push the piston down again. Whenever this happens there is an end of the rarefaction.

While this operation is going on, the air comes out along with the vapour; but we cannot say in what proportion. If it were always uniformly mixed with the vapour, it would diminish rapidly; but this does not appear to be the case. There is a certain period of rarefaction in which a transient cloudiness is perceived in the receiver. This is watery vapour formed at that degree of rarefaction, mingled with, but not dissolved in or united with, the air, otherwise it would be transparent. A similar cloud will appear if damp air be admitted suddenly into an exhausted receiver. The vapour, which formed an uniform transparent mass with the air, is either suddenly expanded, and thus detached from the other ingredient, or is suddenly let go by the air, which expands more than it does. We cannot affirm with probability which of these is the case: different compositions of air, that is, air loaded with vapours from different substances, exhibit remarkable differences in this respect. But we see from this and other phenomena, which shall be mentioned in their proper places, that the air and vapour are not always intimately united; and therefore will not always be drawn out together by the air pump.

But let them be ever so confusedly blended, we see that the air must come out along with the vapour, and its quantity remaining in the receiver must be prodigiously diminished by this association, probably much more than could be, had the receiver only contained pure air.

Let us now consider what must happen in the pear-gage. As the air and vapour are continually drawn off from the receiver, the air in the pear expands and goes off with it. We shall suppose that the generated vapour hinders the gage from rising beyond 29.5. During the continued working of the pump, the air in the pear, whose elasticity is 0.5, slowly mixes with the vapour at the mouth of the pear, and the mixture even advances into its inside, so that if the pumping be long enough continued, what is in the pear is nearly of the same composition with what is in the receiver, consisting perhaps of 20 parts of vapour, and one part of air, all of the elasticity of 0.5. When the pear is plunged into the mercury, and the external air allowed to get into the receiver, the mercury rises in the pear-gage, and leaves not $\frac{1}{60}$, but $\frac{1}{60 \times 20}$ or $\frac{1}{1200}$ of it filled with common air, the vapour having collapsed into an invisible atom of water. Thus the pear-gage will indicate a rarefaction of 1200, while the barometer-gage only showed 60, that is, showed the elasticity of the included substance diminished 60 times. The conclusion to be drawn from these two measures (the one of the rarefaction of air, and the other of the diminution of elasticity) is, that the matter with which the receiver was filled, immediately before the re-admission of the air, consisted of one part of the incondensible air, and $\frac{1200}{60}$, or 20 parts of watery vapour.

The only obscure part of this account is what relates to the composition of the matter which filled the pear-gage before the admission of the mercury. It is not easy to see how the vapour of the receiver comes in by a narrow mouth while the air is coming out by the same passage.

Accordingly it requires a *very long time* to produce this extreme rarefaction in the pear-gage; and there are great irregularities in any two succeeding experiments, as may be seen by looking at Mr Nairne's account of them in the Philosophical Transactions, vol. LXVII. Some vapours appear to have mixed much more readily with the air than others; and there are some unaccountable cases where vitriolic acid and sulphureous bodies were included, in which the diminution of density indicated by the pear-gage was uniformly less than the diminution of elasticity indicated by the barometer-gage. It is enough for us at present to have established, by unquestionable facts, this production of elastic vapour, and the necessity of attending to it, both in the construction of the air-pump and in drawing results from experiments exhibited in it.

Mr Smeaton's pump, when in good order, and perfectly free from all moisture, will in dry weather rarefy air about 600 times, raising the barometer-gage to within $\frac{1}{20}$ of an inch of a fine barometer. This was a performance so much superior to that of all others, and by means of Mr Nairne's experiments opened so new a field of observation, that the air-pump once more became a capital instrument among the experimental philosophers. The causes of its superiority were also so distinct, that artists were immediately excited to a farther improvement of the machine; so that this becomes a new epoch in its history.

There is one imperfection which Mr Smeaton has not attempted to remove. The discharging valve is still opened against the pressure of the atmosphere. An author of the Swedish academy adds a subsidiary pump to this valve, which exhausts the air from above it, and thus puts it in the situation of the piston-valve. We do not find that this improvement has been adopted so as to become general. Indeed, the quantity of air which remains in the passage to this valve is so exceedingly little, that it does not seem to merit attention. Supposing the valve-hole $\frac{1}{20}$

of an inch wide, and as deep (and it need not be more), it will not occupy more than $\frac{1}{9000}$ part of a barrel twelve inches long and two inches wide.

Mr Smeaton, by his ingenious construction, has greatly diminished, but has not annihilated, the obstructions to the passage of the air from the receiver into the barrel. His success encouraged farther attempts. One of the first and most ingenious was that of Professor Russel of the University of Edinburgh, who, about the year 1770, constructed a pump in which both cocks and valves were avoided.

The piston is solid, as represented in Fig. 28. and its rod passes through a collar of leather on the top of the barrel. This collar is divided into three portions by two brass rings *a*, *b*, which leave a very small space round the piston-rod. The upper ring *a* communicates by means of a lateral perforation with the bent tube *l*, *m*, *n*, which enters the barrel at its middle *n*. The lower ring *b* communicates with the bent tube *c*, *d*, which communicates with the horizontal passage *d*, *e*, going to the middle *e* of the pump plate. By the way, however, it communicates also with a barometer-gage *p o*, standing in a cistern of mercury *o*, and covered with a glass tube close at the top. Beyond *e*, on the opposite circumference of the receiver-plate, there is a cock or plug *f* communicating with the atmosphere.

The piston-rod is closely embraced by the three collars of leather; but, as already said, has a free space round it in the two brass rings. To produce this pressure of the leathers to the rod, the brass rings which separate them are turned thinner on the inner side, so that their cross section along a diameter would be a taper wedge. In the side of the piston-rod are two cavities *q r*, *t s*, about one-tenth of an inch wide and deep, and of a length equal to the thickness of the two rings *a*, *b*, and the intermediate collar of leathers. These cavities are so placed on the piston-rod, that when the piston is applied to the bottom of the barrel, the cavity *t s* in the upper end of the rod has its upper end opposite to the ring *a*, and its lower end opposite to the ring *b*, or to the mouth of the pipe *c d*. Therefore, if there be a void in the barrel, the air from the receiver will come from the pipe *c d* into the cavity in the piston-rod, and by it will get past the collar of leather between the rings, and thus will get into the small interstice between the rod and the upper ring, and then into the pipe *l m n*, and into the empty barrel. When the piston is drawn up, the solid rod immediately shuts up this passage, and the piston drives the air through the discharging valve *k*. When it has reached the top of the barrel, and is closely applied to it, the cavity *q r* is in the situation in which *t s* formerly was, and the communication is again opened between the receiver and the empty barrel, and the air is again diffused between them. Pushing down the piston expels the air by the lower discharging-pipe and valve *h i*; and thus the operation may be continued.

This must be acknowledged to be a most simple and ingenious construction, and can neither be called a cock nor a valve. It seems to oppose no obstruction whatever; and it has the superior advantage of rarefying both during the ascent and the descent of the piston, doubling the expedition of the performance, and the operator is not opposed by the pressure of the atmosphere, except towards the end of each stroke. The expedition, however, is not so great as one should expect; for nothing is going on while the piston is in motion, and the operator must stop a while at the end of each stroke, that the air may have time to come through this long, narrow, and crooked passage, to fill the barrel. But the chief difficulty which occurred in the execution arose from the clammy oil with which it was necessary to impregnate the collar of leathers. These were always in a state of strong compression, that they might closely grasp the piston-rod, and prevent all passage of air during the motion of the piston. Whenever therefore the cavities in the piston-rod come into the situations necessary for connecting the receiver and barrel, this oil is squeezed into them, and choaks them up. Hence it always happened that it was some time after the stroke before the air could force its way round the piston-rod, carrying with it the clammy oil which choaked up the tube *l m n*; and when the rarefraction had proceeded a certain length, the diminished elasticity of the air was not able to make its way through these obstructions. The death of the ingenious author put a stop to the improvements by which he hoped to remedy this defect, and we have not heard that any other person has since attempted it. We have inserted it here, because its principle of construction is not only very ingenious, but entirely different from all others, and may furnish very useful hints to those who are much engaged in the construction of pneumatic engines.

In the 73d volume of the Philosophical Transactions, Mr Tiberius Cavallo has given the description of an air-pump contrived and executed by Messrs Haas and Hurter, instrument-makers in London, where these artists have revived Guericke's method of opening the barrel-valve during the last strokes of the pump by a force acting from without. We shall insert so much of this description as relates to this distinguishing circumstance of its construction.

Fig. 26. represents a section of the bottom of the barrel, where AA is the barrel and BB the bottom, which has in its middle a hollow cylinder CCFF, projecting about half an inch into the barrel at CC, and extending a good way downwards to FF. The space between this projection and the sides of the barrel is filled up by a brass ring DD, over the top of which is strained a piece of oiled silk EE, which performs the office of a valve, covering the hole CC. But this hole is filled up by a piece of brass, or rather an assemblage of pieces screwed together GGHHII. It consists of three projecting fillets or shoulders GG, HH, II, which form two hollows between them, and which are filled with rings of oiled leather OO, PP, firmly screwed together. The extreme fillets GG, II, are of equal diameter with the inside of the cylinder, so as to fill it exactly, and the whole stuffed with oiled leather, slide up and down without allowing any air to pass. The middle fillet HH is not so broad, but thicker. In the upper fillet GG there is formed a shallow dish about $\frac{1}{8}$ of an inch deep and $\frac{3}{4}$ wide. This dish is covered with a thin plate, pierced with a grating like Mr Smeaton's valve-plate. There is a perforation VX along the axis of this piece, which has a passage out at one side H, through the middle fillet. Opposite to this passage, and in the side of the cylinder CCFF, is a hole M, communicating with the conduit pipe MN, which leads to the receiver. Into the lower end of the perforation is screwed the pin KL, whose tail L passes through the cap FF. The tail L is connected with a lever RQ, moveable round the joint Q. This lever is pushed upwards by a spring, and thus the whole piece which we have been describing is kept in contact with the slip of oiled silk or valve EE. This is the usual situation of things.

Now suppose a void formed in the barrel by drawing up the piston, the elasticity of the air in the receiver, in the pipe NM, and in the passage XV, will press on the great surface of the valve exposed through the grating, will raise it, and the pump will perform precisely as Mr Smeaton's does. But suppose the rarefaction to have been so long continued, that the air is no longer able to raise the valve; this will be seen by the mercury rising no more in the pump-gage. When this is perceived, the operator must press with his foot on the end R of the lever RQ. This draws down the pin KL, and with it the whole hollow plug with its grated top. And thus, instead of raising the valve from its plate, the plate is here drawn down from the valve. The air now gets in without any obstruction whatever, and the rarefaction proceeds as long as the piston rises. When it is at the top of the barrel, the operator takes his foot from the lever, and the spring presses up the plug again and shuts the valve. The piston-rod passes through a collar of leather, as in Mr Smeaton's pump, and the air is finally discharged through an outward valve in the top of the barrel. These parts have nothing peculiar in them.

This is an ingenious contrivance, similar to what was adopted by Guericke himself; and we have no doubt of these pumps performing extremely well if carefully made: and it seems not difficult to keep the plug perfectly air-tight by supplying plenty of oil to the leathers. We cannot say, however, with precision what may be expected from it, as no account has been given of its effects besides what Mr Cavallo published in the Philosophical Transactions 1783, where he only says, that when it had been long used, it had, in the course of some experiments, rarefied 600 times.

Aiming still at the removing the obstructions to the entry of the air from the receiver into the barrels, Mr Prince, an American, has constructed a pump in which there is no valve or cock whatever between them. In this pump the piston-rod passes through a collar of leathers, and the air is finally discharged through a valve, as in the two last. But we are chiefly to attend, in this place, to the communication between the barrel and the receiver. The barrel widens below into a sort of cistern ABCD (Fig. 30.), communicating with the receiver by the pipe EF. As soon, therefore, as the piston gets into this wider part, where there is a vacancy all round it, the air of the receiver expands freely through the passage FEE into the barrel, in which the descent of the piston had made a void. When the piston is again drawn up, as soon as it gets into the cylindric part of the barrel, which it exactly fills, it carries up the air before it, and expels it by the top valve; and, that this may be done more completely, this valve opens into a second barrel, or air-pump, whose piston is rising at the same time, and therefore the valve of communication (which is the discharging valve of the primary pump) opens with the same facility as Mr Smeaton's piston-valve. While the piston is rising, the air in the receiver expands into the barrel; and when the piston descends, the air in the barrel again collapses till the piston gets again into the cistern, when the air passes out, and fills the evacuated barrel, to be expelled by the piston as before.

No distinct account has as yet been given of the performance of this pump. We only learn that great incoveniences were experienced from the oscillations of the mercury in the gage. As soon as the piston comes into the cistern, the air from the receiver immediately rushes into the barrel, and the mercury shoots up in the gage, and gets into a state of oscillation. The subsequent rise of the piston will frequently keep time with the second oscillation, and increase it. The descent of the piston produces a downward oscillation, by allowing the air below it to collapse; and, by improperly timing the strokes, this oscillation becomes so great as to make the mercury enter the pump. To prevent this, and a greater irregularity of working as a condenser, valves were put in the piston: but as these require force to open them, the addition seemed rather to increase the evil, by rendering the oscillations more simultaneous with the ordinary rate of working. If this could be got over, the construction seems very promising.

It appears, however, of very difficult execution. It has many long, slender, and crooked passages, which must be drilled through broad plates of brass, some of them appearing scarcely practicable. It is rare to find plates and other pieces of brass without air-holes, which it would be very difficult to find out and to close; and it must be very difficult to clear it of obstructions: so that it appears rather a suggestion of theory than a thing warranted by its actual performance.

Mr Lavoisier, or some of the naturalists who were occupied in concert with him in the investigation of the different species of gas which are disengaged from bodies in the course of chemical operations, has contrived an air-pump which has great appearance of simplicity, and, being very different from all others, deserves to be taken notice of

It consists of two barrels *l*, *m*, Fig. 31. with solid pistons *k k*. The pump-plate *a b* is pierced at its centre *c* with a hole which branches towards each of the barrels, as represented by *c d*, *c e*. Between the plate and the barrels slides another plate *h i*, pierced in the middle with a branched hole *f d g*, and near the ends with two holes *h h*, *i i*, which go from its underside to the ends. The holes in these two plates are so adjusted, that when the plate *h i* is drawn so far towards *h* that the hole *i* comes within the barrel *m*, the branch *d f* of the hole in the middle plate coincides with the branch *c d* of the upper plate, and the holes *e*, *g* are shut. Thus a communication is established between the barrel *l* and the receiver on the pump-plate, and between the barrel *m* and the external air. In this situation the barrel *l* will exhaust, and *m* will discharge. When the piston of *l* is at its mouth, and that of *m* touches its bottom, the sliding plate is shifted over to the other side, so that *m* communicates with the receiver through the passage *g d*, *e c*, and *l* communicates with the air by the passages *h h*.

It is evident that this sliding plate performs the office of four cocks in a very beautiful and simple manner, and that if the pistons apply close to the ends of the barrels, so as to expel the whole air, the pump will be perfect. It works, indeed, against the whole pressure of the external air. But this may be avoided by putting valves on the holes *h*, *i*; and these can do no harm, because the air remaining in them never gets back into the barrel till the piston be at the farther end, and the exhaustion of that stroke completed. But the best workmen of London think that it will be incomparably more difficult to execute this cock (for it is a cock of an unusual form), in such a manner that it shall be air-tight, and yet move with tolerable ease, and that it is much more liable to wearing loose than common cocks. No accurate accounts have been received of its performance. It must be acknowledged to be ingenious, and it may suggest to an intelligent artist a method of combining common conical cocks upon one axis so as to answer the same purposes much more effectually; for which reason we have inserted it here.

The last improvement which we shall mention is that published by Mr Cuthbertson, philosophical instrument-maker in Amsterdam. His pump has given such evidences of its perfection, that we can hardly expect or wish for any thing more complete. But we must be allowed to observe, beforehand, that the same construction was invented, and, in part, executed before the end of 1779, by Dr Daniel Rutherford, now professor of botany in the university of Edinburgh, who was at that time engaged in experiments on the production of air during the combustion of bodies in contact with nitre, and who was vastly desirous of procuring a more complete abstraction of pure aerial matter than could be effected by Mr Smeaton's pump. The compiler of this article had then an opportunity of perusing the Doctor's dissertation on this subject, which was read in the Philosophical Society of Edinburgh. In this dissertation the Doctor appears fully apprised of the existence of pure vital air in the nitrous acid as its chief ingredient, and as the cause of its most remarkable phenomena, and to want but a step to the discoveries which have ennobled the name of Mr Lavoisier. He was particularly anxious to obtain *apart* this distinguishing ingredient in its composition, and, for this purpose, to abstract completely from the vessel in which he subjected it to examination, every particle of elastic matter. The writer of this article proposed to him to cover the bottom of Mr Smeaton's pisten with some clammy matter, which should take hold of the bottom valve, and *start it* when the piston was drawn up. A few days after, the Doctor showed him a drawing of a pump, having a conical metal valve in the bottom, furnished with a long slender wire, sliding in the inside of the piston-rod with a gentle friction, sufficient for lifting the valve, and secured against all chance of failure by a spring stop, which took hold of a notch in the inside of the piston-rod about a quarter of an inch from the lower end, so as certainly to lift the valve during the last quarter of an inch of the piston's motion. Being an excellent mechanic, he had executed a valve on this principle, and was fully satisfied with its performance. But having already confirmed his doctrines respecting the nitrous acid by incontrovertible experiments, his wishes to improve the air-pump lost their incitement, and he thought no more of it; and not long after this, the ardour of the philosophers of the Teylerian Society at Haerlem and Amsterdam excited the efforts of Mr Cuthbertson, their instrument-maker, to the same purpose, and produced the most perfect air-pump that has yet appeared. We shall give a description of it, and an account of its performance, in the inventor's own words.

Cuthbertson's Air-Pump.

Plate XI. Fig. is 32. a perspective view of this pump, with its two principal gages screwed into their places. These need not be used together, except in cases where the utmost exactness is required. In common experiments one of them is removed, and a stop-screw put in its place. When the pear-gage is used, a small round plate, on which the receiver may stand, must be first screwed into the hole at A; but this hole is stopped on other occasions with a screw. When all the three gages are used, and the receiver is exhausted, the stop-screw B, at the bottom of the pump, must be unscrewed, to admit the air into the receiver; but when they are not all used, either of the other stop-screws will answer this purpose.

Fig. 33. represents a cross-bar for preventing the barrels from being shaken by working the pump, or by any accident. Its place in Fig. 32. is represented by the dotted lines. It is confined in its place, and kept close down on the barrels by two slips of wood NN, which must be drawn out, as well as the screws OO, when the pump is to be taken asunder.

Plate IX. contains a section of all the working parts of the pump, except the wheel and rack, in which there is nothing uncommon.

Fig. 34. is a section of one of the barrels, with all its internal parts; and Fig. 35, 36, 37, and 38, are different parts of the piston, proportioned to the size of the barrel* and to one another.

In Fig. 34. CD represents the barrel, F the collars of leathers, G a hollow cylindrical vessel to contain oil, R is also an oil-vessel to receive the oil which is drawn, along with the air, through the hole *a a*, when the piston is drawn upwards; and, when this is full, the oil is carried over with the air, along the tube T, into the oil-vessel G. *c c* is a wire which is driven upwards from the hole *a a* by the passage of the air; and as soon as this has escaped, it falls down again by its own weight, shuts up the hole, and prevents all return of the air into the barrel. At *d d* are fixed two pieces of brass, to keep the wire *c c* in a vertical direction, that it may accurately shut the hole. H is a cylindrical wire or rod which carries the piston I, and is made hollow to receive a long wire *g g*, which opens and shuts the hole L; and on the other end of the wire O is screwed a nut, which, by stopping in the narrowest part of the hole, prevents the wire from being driven up too far. This wire and screw are more clearly seen in Fig. 35. and 36; they slide in a collar of leather *r r*, Fig. 35. and 38. in the middle piece of the piston. Fig. 37. and 38. are the two mean parts which compose the piston, and, when the pieces 36. and 39. are added to it, the whole is represented by Fig. 35. Fig. 38. is a piece of brass of a conical form, with a shoulder at the bottom. A long hollow screw is cut in it, about ⅔ of its length, and the remainder of the hole, in which there is no screw, is of about the same diameter with the screwed part, except a thin plate at the end, which is of a width exactly equal to the thickness of *g g*. That part of the inside of the conical brass in which no thread is cut, is filled with oiled leathers with holes, through which *g g* can slide stiffly. There is also a male screw with a hole in it, fitted to *g g*, serving to compress the leathers *r r*. In Fig. 37. *aaaa* is the outside of the piston, the inside of which is turned so as exactly to fit the outside of Fig. 38. *b b* are round leathers about 60 in number, *c c* is a circular piece of brass of the size of the leathers, and *d d* is a screw serving to compress them. The screw at the end of Fig. 36. is made to fit the screw in Fig. 38. Now if Fig. 39. be pushed into Fig. 38. this into Fig. 37. and Fig. 36. be screwed into the end of Fig. 38, these will compose the whole of the piston, as represented in Fig. 35. H in Fig. 34. represents the same part as H in Fig. 35. and is that to which the rack is fixed. If, therefore, this be drawn upwards, it will cause Fig. 38. to shut close into Fig. 37, and drive out the air above it; and when it is pushed downward, it will open as far as the shoulder *a a* will permit, and suffer air to pass through. AA Fig. 40. is the receiver plate, BB is a long square piece of brass, screwed into the under side of the plate, through which a hole is drilled corresponding to that in the centre of the receiver-plates and with three female screws, *b*, *b*, *c*.

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* The piston and barrel are 1,65 inches in diameter, in proportion to which the scale is drawn. Figures 35, 36, 37, 38, are, however, of double size.

The rarefaction of the air in the receiver is effected as follows:—Suppose the piston at the bottom of the barrel. The inside of the barrel, from the top of the piston to *a*, contains common air. When the rod is drawn up, the upper part of the piston sticks fast in the barrel till the conical part connected with the rod shuts the conical hole, and its shoulder applies close to its bottom. The piston is now shut, and therefore the *whole* is drawn up by the rack-work, driving the air before it through the hole *a a*, into the oil-vessel at R, and out into the room by the tube T. The piston will then be at the top of the barrel at *a*, and the wire *g g* will stand nearly as represented in the figure just raised from the hole L, and prevented from rising high by the nut O. During this motion the air will expand in the receiver, and come along the bent tube *m* into the barrel. Thus the barrel will be filled with air, which, as the piston rises, will be rarefied in proportion as the capacity of the receiver, pipes, and barrel, is to the barrel alone. When the piston is moved down again by the rack-work, it will force the conical part, Fig. 38. out of the hollow part, Fig. 37. as far as the shoulders *a a*; Fig. 35. will rest on *a a*, Fig. 37. which will then be so far open as to permit the air to pass freely through it, while at the same time the end of *g g* is forced against the top of the hole, and shuts it in order to prevent any air from returning into the receiver. Thus the piston, moving downwards, suffers the air to pass out between the sides of Fig. 37. and 38.; and, when it is at the bottom of the barrel, will have the column of air above it; and consequently, when drawn upwards it will shut, and drive out this air, and, by opening the hole L at the same time, will give a free passage to more air from the receiver. This process being continued, the air of the receiver will be rarefied *as far as its expansive power will permit.* For in this machine there are no valves to be forced open by the elasticity of the air in the receiver, which at last it is unable to effect.

There is therefore nothing to prevent the air from expanding to its utmost degree.

It may be suspected here, that as the air must escape through the discharging passage *a c*, Plate XI. Fig. 34. against the pressure of a column of oil and the weight of the wire, there will remain in this passage a quantity of air of considerable density, which will expand again into the barrel during the descent of the piston, and thus put a stop to the progress of rarefaction. This is the case in Mr Smeaton's pump, and all which have valves in the piston. But it is the peculiar excellency of this pump, that whatever be the density of the air remaining in *a c*, the rarefaction will still go on. It is worth while to be perfectly convinced of this. Let us suppose that the air contained in *a c* is $\frac{1}{100}$ part of the common air which would fill the barrel, and that the capacity of the barrel is equal to that of the receiver and passages, and that the air in the receiver and barrel is of the same density, the piston being at the bottom of the barrel. The barrel will therefore contain $\frac{10}{1000}$ parts of its natural quantity, and the receiver $\frac{10}{1000}$. Now let the piston be drawn up. No air will be discharged at *a c*, because it will contain the whole air which was in the barrel, and which has now collapsed into its ordinary bulk. But this does not in the least hinder the air of the receiver from expanding into the barrel, and diffusing itself equally between both. Each will now contain $\frac{5}{1000}$ of their ordinary quantity when the piston is at the top, and *a c* will contain $\frac{1}{100}$ as before, or $\frac{10}{1000}$. Now push down the piston. The hole L is instantly shut, and the air in *a c* expands into the barrel, and the barrel now contains $\frac{15}{1000}$. When the piston has reached the bottom, let it be again drawn up. There will be $\frac{5}{1000}$ discharged through *c*, and the air in the receiver will again be equally distributed between it and the barrel. Therefore the receiver will now contain $\frac{2\frac{1}{2}}{1000}$. When the piston reaches the bottom, there will be $\frac{12\frac{1}{2}}{1000}$ in the barrel. When again drawn up to the top, there will be $\frac{2\frac{1}{2}}{1000}$ discharged, and the receiver will contain $\frac{1\frac{1}{4}}{1000}$; and when the piston reaches the bottom, there will be $\frac{11\frac{1}{4}}{1000}$. At the next stroke the receiver will contain only $\frac{0.\frac{5}{8}}{1000}$, &c. &c.

Thus it appears, that notwithstanding the $\frac{10}{1000}$ which always expands back again out of the hole *a c* into the barrel, the rarity of the air in the receiver will be doubled at every stroke. There is therefore no need of a subsidiary air-pump at *c*, as in the American air-pump, and in the Swedish attempt to improve Smeaton's.

In using this air-pump no particular directions are necessary, nor is any peculiar care necessary for keeping it in order, except that the oil-vessel A be always kept about half full of oil. When the pump has stood long without being used, it will be proper to draw a table-spoonful of olive-oil through it, by pouring it into the hole in the middle of the receiver-plate when the piston is at the bottom of the barrel. Then by working the piston, the oil will be drawn through all the parts of the pump, and the surplus will be driven through the tube T into the oil-vessel G. Near the top of the piston-rod at H there is a hole which lets some oil into the inside of the rod, which gets at the collar of leathers *r r*, and keeps the wire *g g* air-tight.

When the pump is used for condensation at the same time that it rarefies, or separately, the piece containing the bent tube T must be removed, and Fig. 41. put into its place, and fixed by its screws. Fig. 41. as drawn in the plate, is intended for a double-barrelled pump. But for a single barrel only one piece is used, represented by *b a a*, the double piece being cut off at the dotted line *a a*. In this piece is a female screw to receive the end of a long brass tube, to which a bladder (if sufficient for the experiment of condensation), or a glass, properly secured for this purpose, must be screwed. Then the air which is abstracted from the receiver on the pump-plate will be forced into the bladder or glass. But if the pump be double, the apparatus Fig. 41. is used, and the long brass tube screwed on at *c*.

Fig. 42. and 43. represent the two gages, which will be sufficiently explained afterwards. Fig. 42. is screwed into *c b*, or into the screw at the other end of *c* Fig. 40. and Fig. 43. into the screw *a b* Fig. 40.

If it be used as a single pump, either to rarefy or condense, the screw K, which fastens the rack to the piston-rod H, must be taken out. Then turning the winch till H is depressed as low as possible, the machine will be fitted to exhaust as a single pump; and if it be required to condense, the direction formerly given must be observed with regard to the tube T and Fig. 42.

" I took (says Mr Cuthbertson) two barometer-tubes of an equal bore with that fixed to the pump. These were filled with mercury four times boiled. They were then compared, and stood exactly at the same height. The mercury in one of them was boiled in it four times more, without making any change in their height; they were therefore judged very perfect. One of these was immersed in the cistern of the pump-gage, and fastened in a position parallel to it, and a sliding scale of one inch was attached to it. This scale, when the gage is used, must have its upper edge set equal with the surface of the mercury in the boiled tube after exhaustion, and the difference between the height of the mercury in this and in the other barometer-tube may be observed to the $\frac{1}{100}$ of an inch; and being close together, no error arises from their not being exactly vertical, if they are only parallel. This gage will be better understood by inspecting Fig. 43.

"I used a second gage, which I shall call a double syphon.—See plate XI. Fig. 42. This was also prepared with the utmost care. I had a scale for measuring the difference between the height of the columns in the two legs. It was an inch long, and divided as the former, and kept in a truly vertical position by suspending it from a point with a weight hung to it, as represented in the figure. Upon comparing these two gages, I always found them to indicate the same degree of rarefaction. I also used a pear-gage, though the most imperfect of all, in order to repeat the curious experiments of Mr Nairne and others."

When experiments require the utmost rarefying power of the pump, the receiver must not be placed on leather, either oiled or soaked in water, as is usually done. The pump-plate and the edge of the receiver must be ground very flat and true, and this with very fine emery, that no roughness may remain. The plate of the pump must then be wiped very clean and very dry, and the receiver rubbed with a warm cloth till it become electrical. The receiver being now set on the plate, hog's-lard, either alone or mixed with a little oil, which has been cleared of water by boiling, must be smeared round its outside edge. In this condition the pump will rarefy its utmost, and what still remains in the receiver will be permanent air. Or a little of this composition may be thinly smeared on the pump-plate; this will prevent all risk of scratching it with the edge of the receiver. Leather of very uniform thickness, long dried before a fire, and well soaked in this composition, which must be cleared of all water by the first boiling, will answer very well, and is expeditious, when receivers are to be frequently shifted. Other leathers should be at hand soaked in a composition containing a little rosin. This gives it a clamminess which renders it impermeable to air, and is very proper at all joints of the pump, and all apparatus for pneumatic experiments. As it is impossible to render the pear-gage as dry as other parts of the apparatus, there will be generally some variation between this and the other gages.

When it is only intended to show the utmost power of the pump, without intending to ascertain the quality of the residuum, the receiver may be set on wet leather. If, in this condition, the air be rarefied as far as possible, the syphon and barometer-gage will indicate a less degree of rarefaction than in the former experiments. But when the air is let in again, the pear-gage will point out a rarefaction some thousands of times greater than it did before. If the true quality of permanent air after exhaustion be required, the pear-gage will be nearest the truth: for when the air is rarefied to a certain degree, the moistened leather emits an expansible fluid, which, filling the receiver, forces out the permanent air; and the two first gages indicate a degree of exhaustion which relates to the whole elastic matter remaining in the receiver, viz. to the expansible fluid together with the permanent air; whereas the pear-gage points out the degree of exhaustion, with relation to the permanent air alone, which remains in the receiver; for by the pressure of the air admitted into the receiver, the elastic vapour is reduced to its former bulk, which is imperceptible.

Many bodies emit this elastic fluid when the pressure of the air is much diminished; a piece of leather, in its ordinary damp state, about an inch square, or a bit of green or dry wood, will supply this for a great while.

When such fluids have been generated in any experiments, the pump must be carefully cleared of them, for they remain not only in the receiver, but in the barrels and passages, and will again expand when the exhaustion has been carried far.

The best method of clearing the pump is to take a very large receiver, and use every precaution to exhaust it as far as possible. Then the expansible matter lurking in the barrels and passes will be diffused through the receiver also, or will be carried off along with its air. It will be as much rarer than it was before, as the aggregate capacity of the receiver barrels and passes is larger than that of the two last.

The performance of the pump may be judged of from the four following experiments:—

The two gages being screwed into their places, and the hole in the receiver-plate shut up, the pump was made to exhaust as far as it could. The mercury in the legs of the syphon was only $\frac{1}{40}$ of an inch out of the level, and that in the boiled barometer-tube $\frac{1}{40}$ of an inch higher than in the one screwed to the pump. A standard barometer then stood at 30 inches, and therefore the pump rarefied the permanent air 1200 times. This is twice as much as Mr Nairne found Mr Smeaton's do in its best state. Mr Cavallo seems disposed to give a favourable (while we must suppose it a just) account of Haas and Hurter's pump, and it appears never to have exceeded 600 times. Mr Cuthbertson has often found the mercury within $\frac{1}{100}$ of an inch of the level in the syphon-gage, indicating a rarefaction of 3000.

To one end of a glass tube, 2 inches diameter and 30 inches long, was fitted a brass cap and collar of leather, through which a wire was inserted, reaching about two inches within the tube. This was connected with the conductor of an electric machine. The other end was ground flat and set on the pump-plate. When the gages indicated a rarefaction of 300, the light became steady and uniform, of a pale colour, though a little tinged with purple; at 600 the light was of a pale dusky white; when 1200 it disappeared in the middle of the tube, and the tube conducted so well that the prime conductor only gave sparks so faint and short as to be scarcely perceptible. After taking off the tube, and making it as dry as possible, it was again connected with the conductor, which was giving sparks two inches long. When the air in it was rarefied ten times, the sparks were of the same length. Sometimes a pencil of light darted along the tube. When the rarefaction was 20, the spark did not exceed an inch, and light streamed the whole length of the tube. When the rarefaction was 30, the sparks were half an inch; and the light rushed along the tube in great streams. When the rarefaction was 100, the sparks were about $\frac{1}{8}$ of an inch long, and the light filled the tube in an uninterrupted body. When 300, the appearances were as before. When 600, the sparks were $\frac{1}{18}$, and the light was of a faint white colour in the middle, but tinged with purple towards the ends. When 1200, the light was hardly perceptible in the middle, and was much fainter at the ends than before, but still ruddy. When 1400, which was the most the pump could produce, six inches of the middle of the tube were quite dark, and the ends free of any tinge of red, and the sparks did not exceed $\frac{1}{40}$ of an inch.

Although this noble instrument originated in Germany, all its improvements were made in this kingdom. Both the mechanical and pneumatical principles of Mr Boyle's construction were extremely different from the German, and, in respect of expedition and conveniency, much superior. The double barrel and gage by Hawkesbee were capital improvements, and on principle; and Mr Smeaton's method of making the piston work in rarefied air made a complete change in the whole process.

Aided by this machine, we can make experiments establishing and illustrating the gravity and elasticity of the air in a much more perspicuous manner than could be done by the spontaneous phenomena of nature.

It allows us in the first place to show the materiality of air in a very distinct manner. Bodies cannot move about in the atmosphere without displacing it. This requires force; and the resistance of the air always diminishes the velocity of bodies moving in it. A heavy body therefore has the velocity of its fall diminished; and if the quantity of air displaced be very great, the diminution will be very considerable. This is the reason why light bodies, such as feathers, fall very slowly. Their moving force is very small, and can therefore displace a great quantity of air only with a very small velocity. But if the same body be dropped *in vacuo*, when there is no air to be displaced, it falls with the whole velocity competent to its gravity. Fig. 44. Plate XII. represents an apparatus by which a guinea and a downy feather are dropped at the same instant, by opening the forceps which holds them by means of the slip-wire in the top of the receiver. If this be done after the air has been pumped out, the guinea and the feather will be observed to reach the bottom at the same instant.

Fig. 45. represents another apparatus for showing the same thing. It consists of two sets of brass vanes put in separate axles, in the manner of windmill sails. One set has their edges placed in the direction of their whirling motion, that is, in a plane to which the axis is perpendicular. The planes of the other set pass through the axis, and they are therefore trimmed so as directly to front the air through which they move. Two springs act upon pins projecting from the axis; and their strength or tensions are so adjusted, that when they are disengaged *in vacuo*, the two sets continue in motion equally long. If they are disengaged in the air, the vanes which beat the air with their planes will stop long before those which cut it edgewise.

We can now abstract the air almost completely from a dry vessel, so as to know the precise weight of the air which filled it. The first experiment we have of this kind, done with accuracy, is that of Dr Hooke, Feb. 10, 1664, when he found 114 pints of air to weigh 945 grains. One pint of water was $8\frac{7}{32}$ ounces. This gives for the specific gravity of air $\frac{1}{840}$ very nearly.

Since we are thus immersed in a gravitating fluid, it follows, that every body preponderates only with the excess of its own weight above that of the air which it displaces; for every body loses by this immersion the weight of the displaced air. A cubic foot loses about 521 grains in frosty weather. We see balloons even rise in the air, as a piece of cork rises in water. A mass of water which really contains 850 pounds will load the scale of a balance with 849 only, and will be balanced by about 849$\frac{1}{8}$ pounds of brass. This is evinced by a very pretty experiment, represented in Fig. 46. A small beam is suspended within a receiver. To one end of the beam is appended a thin glass or copper ball, close in every part. This is balanced by a small piece of lead hung on the other arm. As the air is pumped out of the receiver, the ball will gradually preponderate, and will regain its equilibrium when the air is re-admitted.

There is a case in which this observation is of consequence to the philosopher: we mean the measuring of time by pendulums. As the accelerating force on a pendulum is not its whole weight, but the excess of its weight over that of the displaced air, it follows that a pendulum will vibrate more slowly in the air than *in vacuo*. A pendulum composed of lead, iron, and brass, may be about 8400 times heavier than the air which it displaces when the barometer is at 30 inches and the thermometer at 32°, and the accelerating force will be diminished about $\frac{1}{15800}$. This will cause a second pendulum to make about five vibrations less in a day than it would do *in vacuo*. In order therefore to deduce the accelerative power of gravity from the length of a pendulum vibrating in the air, we must make an allowance of 0″,17, or $\frac{17}{100}$ of a second, per day, for every inch that the barometer stands lower than 30 inches. But we must also note the temperature of the air; because, when the air is warm, it is less dense when supporting by its elasticity the same weight of atmosphere, and we must know how much its density is diminished by an increase of temperature. The correction is still more complicated; for the change of density affects the resistance of the air, and this affects the time of the vibration, and this by a law that is not yet well ascertained. As far as we can determine from any experiments that have been made, it appears that the change arising from the altered resistance takes off about $\frac{4}{5}$ of the change produced by the altered density, and that a second pendulum makes but three vibrations a-day more *in vacuo* than in the open air. This is a very unexpected result; but it must be owned that the experiments have neither been numerous nor very nicely made.

The air-pump also allows us to show the effects of the air's pressure in a great number of amusing and instructive phenomena.

When the air is abstracted from the receiver, it is strongly pressed to the pump-plate by the incumbent atmosphere, and it supports this great pressure in consequence of its circular form. Being equally compressed on all sides, there is no place where it should give way rather than another; but if it be thin, and not very round, which is sometimes the case, it will be crushed to pieces. If we take a square thin phial, and apply an exhausting syringe to its mouth, it will not fail being crushed.

As the operation of pumping is something like sucking, many of these phenomena are in common discourse ascribed to suction, a word much abused; and this abuse misleads the mind exceedingly in its contemplation of natural phenomena. Nothing is more usual than to speak of the suction of a syringe, the suction and draught of a chimney, &c. The following experiment puts the true cause of the strong adhesion of the receiver beyond a doubt:

Place a small receiver or cupping-glass on the pump-plate, without covering the central hole, as represented in Fig. 47, and cover it with a larger receiver. Exhaust the air from it; then admit it as suddenly as possible. The outer receiver, which after the rarefaction adhered strongly to the plate, is now loose, and the cupping-glass will be found sticking fast to it. While the rarefaction was going on, the air in the small receiver also expanded, escaped from it, and was abstracted by the pump. When the external air was suddenly admitted, it pressed on the small receiver, and forced it down to the plate, and thus shut up all entry. The small receiver must now adhere; and there can be no suction, for the pipe of the pump was on the outside of the cupping-glass.

This experiment sometimes does not succeed, because the air finds a passage under the brim of the cupping-glass. But if the cupping-glass be pressed down by the hand on the greasy leather or plate, every thing will be made smooth, and the glass will be so little raised by the expansion of its air during the pumping, that it will instantly clap close when the air is re-admitted.

In like manner, if a thin square phial be furnished with a valve, opening from within, but shutting when pressed from without, and if this phial be put under a receiver, and the air be abstracted from the receiver, the air in the phial will expand during the rarefaction, will escape through the valve, and be at last in a very rarefied state within the phial. If the air be now admitted into the receiver, it will press on the flat sides of the included phial, and crush it to pieces. See Fig. 48.

If a piece of wet ox-bladder be laid over the top of a receiver whose orifice is about four inches wide, and the air be exhausted from within it, the incumbent atmosphere will press down the bladder into a hollow form, and then burst it inward with a prodigious noise. See Fig. 49. Or if a piece of thin flat glass be laid over the receiver, with an oiled leather between them to make the juncture air-tight, the glass will be broken downwards. This must be done with caution, because the pieces of glass sometimes fly about with great force.

If there be formed two hemispherical cups of brass, with very flat thick brims, and one of them be fitted with a neck and stop-cock, as represented by Fig. 50. the air may be abstracted from them by screwing the neck into the hole in the pump-plate. To prevent the insinuation of air, a ring of oiled leather may be put between the rims. Now unscrew the sphere from the pump, and six hooks to each, and suspend them from a strong nail, and hang a scale to the lowest. It will require a considerable weight to separate them; namely, about 15 pounds for every square inch of the great circle of the sphere. If this be four inches diameter, it will require near 190 pounds. This pretty experiment was first made by Otto Guericke, and on a very great scale. His sphere was of a large size, and, when exhausted, the hemispheres could not be drawn asunder by 20 horses. It was exhibited, along with many others equally curious and magnificent, to the Emperor of Germany and his court, at the breaking up of the diet of Ratisbon in 1654.

If the loaded syringe mentioned in No 16. be suspended by its piston from the hook in the top plate of the receiver, as in Fig. 51. and the air be abstracted by the pump, the syringe will gradually descend (because the elasticity of the air, which formerly balanced the pressure of the atmosphere, is now diminished by its expansion, and is therefore no longer able to press the syringe to the piston), and it will at last drop off. If the air be admitted before this happens, the syringe will immediately rise again.

Screw a short brass pipe into the neck of a transporter, on which is set a tall receiver, and immerse it into a cistern of water. On opening the cork the pressure of the air on the surface of the water in the cistern will force it up through the pipe, and cause it to spout into the receiver with a strong jet, because there is no air within to balance by its elasticity the pressure of the atmosphere. See Fig. 52.

It is in the same way that the gage of the air-pump performs its office. The pressure of the atmosphere raises the mercury in the gage, till the weight of the mercury, together with the remaining elasticity of the air in the receiver, are in equilibrio with the whole pressure of the atmosphere: therefore the height and weight of the mercury in the gage is the excess of the weight of the atmosphere above the elasticity of the included air; and the deficiency of this height from that of the mercury in the Toricellian tube is the measure of this remaining elasticity.

If a Toricellian tube be put under a tall receiver, as shown in Fig. 53. and the air be exhausted, the mercury in the tube will descend while that in the gage will rise; and the sum of their heights will always be the same, that is, equal to the height in an ordinary barometer. The height of the mercury in the receiver is the effect and measure of the remaining elasticity of the included air, and the height in the pump-gage is the unbalanced pressure of the atmosphere. This is a very instructive experiment, perfectly similar to Mr Auzout's, formerly mentioned, and completely establishes and illustrates the whole doctrine of atmospheric pressure.

We get a similar illustration and confirmation (if such a thing be now needed) of the cause of the rise of water in pumps, by screwing a syringe into the top plate of a receiver, which syringe has a short glass pipe plunging into a small cup of water. See Fig. 54. When the piston-rod is drawn up, the water rises in the glass pipe, as in any other pump, of which this is a miniature representation. But if the air has been previously exhausted from the receiver, there is nothing to press on the water in the little jar; and it will not rise in the glass pipe though the piston of the syringe be drawn to the top.

Analogous to the rise of water in pumps is its rise and motion in syphons. Suppose a pipe ABCD, Fig. 55. bent at right angles at B and C, and having its two ends immersed in the cisterns of water A and D. Let the leg CD be longer than the leg BA, and let the whole be full of water. The water is pressed upwards at A with a force equal to the weight of the column of air EA reaching to the top of the atmosphere; but it is pressed downwards by the weight of the column of water BA. The water at E is pressed downwards by the weight of the column CD, and upwards by the weight of the column of air FD reaching to the top of the atmosphere. The two columns of air differ very little in their weight, and may without any sensible error be considered as equal. Therefore there is a superiority of pressure downwards at D, and the water will flow out there. The pressure of the air will raise the water in the leg AB, and thus the stream will be kept up till the vessel A is emptied as low as the orifice of the leg BA, provided the height of AB is not greater than what the pressure of the atmosphere can balance, that is, does not exceed 32 or 33 feet for water, 30 inches for mercury, &c.

A syphon then will always run from that vessel whose surface is highest; the form of the pipe is indifferent, because the hydrostatical pressures depend on the vertical height only. It must be filled with water by some other contrivance, such as a funnel, or a pump applied a-top; and the funnel must be stopped up, otherwise the air would get in, and the water would fall in both legs.

If the syphon have equal legs, as in Fig. 56. and be turned up at the ends, it will remain full of water, and be ready for use. It need only be dipped into any vessel of water, and the water will then flow out at the other end of the syphon. This is called the *Wirtemberg syphon*, and is represented in Fig. 56.

What is called the *syphon fountain*, constructed on this principle, is shown in Fig. 57. where AB is a tall receiver, standing in a wide basin DE, which is supported on the pedestal H by the hollow pillar FG. In the centre of the receiver is a jet pipe C, and in the top a ground stopper A. Near the base of the pillar is a cock N, and in the pedestal is another cock O.

Fill the basin DE with water within half an inch of the brim. Then pour in water at the top of the receiver (the cock N being shut) till it is about half full, and then put in the stopper. A little water will run out into the vessel DE. But before it runs over, open the cock N, and the water will run into the cistern H; and by the time that the pipe C appears above water, a jet will rise from it, and continue as long as water is supplied from the basin DE. The passage into the base cistern may be so tempered by the cock N that the water within the receiver shall keep at the same height, and what runs into the base may be received from the cock O into another vessel, and returned into DE, to keep up the stream.

This pretty philosophical toy may be constructed in the following manner. BB, Fig. 58, is the ferril or cap into which the receiver is cemented. From its centre descends the jet pipe C *a*, sloping outwards, to give room for the discharging-pipe *b d* of larger diameter, whose lower extremity *d* fits tightly into the top of the hollow pillar FG.

The operation of the toy is easily understood. Suppose the distance from C to H (No 1.) three feet, which is about $\frac{1}{12}$ of the height at which the atmosphere would support a column of water. The water poured into AB would descend through FG (the hole A being shut) till the air has expanded $\frac{1}{10}$, and then it would stop. If the pipe C*a* be now opened, the pressure of the air on the surface of the water in the cistern DE will cause it to spout through C to the height of three feet nearly, and the water will continue to descend through the pipe FG. By tempering the cock N so as to allow the water to pass through it as fast as it is supplied by the jet, the amusement may be continued a long time. It will stop at last, however; because, as the jet is made into rarefied air, a little air will be extricated from the water, which will gradually accumulate in the receiver, and diminish its rarefaction, which is the moving cause of the jet. This indeed is an inconvenience felt in every employment of syphons, so much the more remarkably as their top is higher than the surface of the water in the cistern of supply.

Cases of this employment of a syphon are not unfrequent. When water collected at A (Fig. 59.) is to be conducted in a pipe to C, situated in a lower part of the country, it sometimes happens, as between Lochend and Leith, that the intervening ground is higher than the fountain-head as at B. A forcing pump is erected at A, and the water forced along the pipe. Once it runs out at C, the pump may be removed, and the water will continue to run on the syphon principle, provided BD do not exceed 33 feet. But the water in that part of the conduit which is above the horizontal plane AD, is in the same state as in a receiver of rarefied air, and gives out some of the air which is chemically united with it. This gradually accumulates in the elevated part of the conduit, and at last chokes it entirely. When this happens, the forcing pump must again be worked. Although the elevation in the Leith conduit is only about eight or ten feet, it will seldom run for 12 hours. N. B. This air cannot be discharged by the usual air-cocks; for if there were an opening at B, the air would rush in, and immediately stop the motion.

This combination of air with water is very distinctly seen by means of the air-pump. If a small glass containing cold water, fresh drawn from the spring, be exposed, as in

Fig. 60. under the receiver, and the air rarefied, small bubbles will be observed to form on the inner surface of the glass, or on the surface of any body immersed in it, which will increase in size, and then detach themselves from the glass and reach the top; as the rarefaction advances, the whole water begins to show very minute air-bubbles rising to the top; and this appearance will continue for a very long time, till it be completely disengaged. Warming the water will occasion a still farther separation of air, and a boiling heat will separate all that can be disengaged. The reason assigned for these air-bubbles first appearing on the surface of the glass, &c. is, that air is attracted by bodies, and adheres to their surface. This may be so. But it is more probably owing to the attraction of the water for the glass, which causes it to quit the air which it held in solution, in the same manner as we see it happen when it is mixed with spirits of wine, with vitriolic acid, &c. or when salts or sugar are dissolved in it. For if we pour out the water which has been purged of air by boiling *in vacuo*, and fill the glass with fresh water, we shall observe the same thing, although a film of the purified water was left adhering to the glass. In this case, there can be no air adhering to the glass.

Water thus purged of air by boiling (or even without boiling) *in vacuo*, will again absorb air when exposed to the atmosphere. The best demonstration of this is to fill with this water a phial, leaving about the size of a pea not filled. Immerse this in a vessel of water, with the mouth undermost, by which means the air-bubble will mount up to the bottom of the phial. After some days standing in this condition, the air-bubble will be completely absorbed, and the vessel quite filled with water.

The air in this state of chemical solution has lost its elasticity, for the water is not more compressible than common water. It is also found that water brought up from a great depth under ground contains much more air than water at the surface. Indeed fountain waters differ exceedingly in this respect. Other liquors contain much greater quantities of elastic fluids in this loosely combined state. A glass of beer treated in the same way will be almost wholly converted into froth by the escape of its fixed air, and will have lost entirely the prickling smartness which is so agreeable, and it become quite vapid.

The air-pump gives us, in the next place, a great variety of experiments illustrative of the air's elasticity and expansibility. The very operation of exhaustion, as it is called, is an instance of its great, and hitherto unlimited, expansibility. But this is not palpably exhibited to view.—The following experiments shew it most distinctly:

1. Put a flaccid bladder, of which the neck is firmly tied with a thread, under a receiver, and work the pump. The bladder will gradually swell, and will even be fully distended. Upon readmitting the air into the receiver, the bladder gradually collapses again into its former dimensions: while the bladder is flaccid, the air within it is of the same density and elasticity with the surrounding air, and its elasticity balances the pressure of the atmosphere. When part of the air of the receiver is abstracted, the remainder expands so as still to fill the receiver: but by expanding its elasticity is plainly diminished; for we see by the fact, that the elasticity of the air of the receiver no longer balances the elasticity of that in the bladder, as it no longer keeps it in its dimensions. The air in the bladder expands also: it expands till its diminished elasticity is again in equilibrio with the diminished elasticity of the air in the receiver; that is, till its density is the same. When all the wrinkles of the bladder have disappeared, its air can expand no more, although we continue to diminish the elasticity of the air of the receiver by further rarefaction. The bladder now tends to burst; and if it be pierced by a point or knife fastened to the slip-wire, the air will rush out, and the mercury descend rapidly in the gage.

If a phial or tube be partly filled with water, and immersed in a vessel of water with the mouth downwards, the air will occupy the upper part of the phial. If this apparatus be put under a receiver, and the air be abstracted, the air in the phial will gradually expand, allowing the water to run out by its weight till the surface of the water be on a level within and without. When this is the case, we must grant that the density and elasticity of the air in the phial is the same with that in the receiver. When we work the pump again, we shall observe the air in the phial expand still more, and come out of the water in bubbles. Continuing the operation, we shall see the air continually escaping from the phial: when this is over, it shows that the pump can rarefy no more. If we now admit the air into the receiver, we shall see the water rise into the phial, and at last almost completely fill it, leaving only a very small bubble of air at top. This bubble had expanded so as to fill the whole phial. See this represented in Fig. 61.

Every one must have observed a cavity at the big end of an egg between the shell and the white. The white and yolk are contained in a thin membrane or bladder which adheres loosely to the shell, but is detached from it at that part; and this cavity increases by keeping the egg in a dry place. One may form a judgment of its size, and therefore of the freshness of the egg, by touching it with the tongue; for the shell, where it is not in contact with the contents, will presently feel warm, being quickly heated by the tongue, while the rest of the egg will feel cold.

If a hole be made in the opposite end of the egg, and it be set on a little tripod, and put under a receiver, the expansion of the air in the cavity of the egg will force the contents through the hole till the egg be quite emptied: or, if nearly one half of the egg be taken away at the other end, and the white and yolk taken out, and the shell be put under a receiver, and the air abstracted, the air in the cavity of the egg will expand, gradually detaching the membrane from the shell, till it causes it to swell out; and gives the whole the appearance of an entire egg. In like manner shrivelled apples and other fruits will swell in vacuo by the expansion of the air confined in their cavities.

If a piece of wood, a twig with green leaves, charcoal, plaster of Paris, &c. be kept under water in vacuo, a prodigious quantity of air will be extracted; and if we readmit the air into the receiver, it will force the water into the pores of the body. In this case the body will not swim in water as it did before, shewing that the vegetable fibres are specifically heavier than water. It is found, however, that the air contained in the pith and bark, such as cork, is not all extricated in this way; and that much of it is contained in vesicles which have no outlet: being secreted into them in the process of vegetation, as it is secreted into the air-bladder of fishes, where it is generally found in a pretty compressed state, considerably denser than the surrounding air. The air-bladder of a fish is surrounded by circular and longitudinal muscles, by which the fish can compress the air still further; and, by ceasing to act with them, allow it to swell out again. It is in this manner that the fish can suit its specific gravity to its situation in the water, so as to have no tendency either to rise or sink: but if the fish be put into the receiver of an air-pump, the rarefaction of the air obliges the fish to act more strongly with these contracting muscles, in order to adjust its specific gravity; and if too much air has been abstracted from the receiver, the fish is no longer able to keep its air-bladder in the proper degree of compression. It becomes therefore too buoyant, and comes to the top of the water, and is obliged to struggle with its tail and fins in order to get down; frequently in vain. The air-bladder sometimes bursts, and the fish goes to the bottom, and can no longer keep above without the continual action of its tail and fins. When fishes die, they commonly float at top, their contractive action being now at an end. All this may be illustrated (but very imperfectly) by a small half-blown bladder, to which is appended a bit of lead, just so heavy as to make it sink in water; when this is put under a receiver, and the air abstracted, the bubble will rise to the top; and, by nicely adjusting the rarefaction, it may be kept at any height.—See Fig. 46.

The playthings called *Cartesian devils* are similar to this: they are hollow glass figures, having a small aperture in the lower part of the figures, as at the point of the foot; their weight is adjusted so that they swim upright in water. When put into a tall jar filled to the top, and having a piece of leather tied over it, they will sink in the water, by pressing on the leather with the ball of the hand: this, by compressing the water, forces some of it to enter into the figure, and makes it heavier than the water; for which reason it sinks, but rises again on removing the pressure of the hand.—See Fig. 47, No 1. and 2.

If a half-blown ox-bladder be put into a box, and great weights laid on it, and the whole be put under a receiver, and the air abstracted, the air will, by expanding, lift up the weights, though above an hundred pounds.——See Fig. 48.

By such experiments the great expansibility of the air is abundantly illustrated, as its compressibility was formerly by means of the condensing syringe. We now see that the two sets of experiments form an uninterrupted chain; and that there is no particular state of the air's density where the compressibility and expansibility is remarkably dissimilar. Air in its ordinary state expands; because its ordinary state is a state of compression by the weight of the atmosphere: and if there were a pit about 33 miles deep, the air at the bottom would probably be as dense as water; and if it were 50 miles deep, it would be as dense as gold, if it did not become a liquid before this depth: nay, if a bottle with its mouth undermost were immersed six miles under water, it would probably be as dense as water; we say probably, for this depends on the nature of its compressibility; that is, on the relation which subsists between the compression and the force which produces it.

This is the circumstance of its constitution, which we now proceed to examine; and it is evidently a very important circumstance. We have long ago observed, that the great compressibility and permanent fluidity of air, observed in a vast variety of phenomena, is totally inexplicable, on the supposition that the particles of air are like so many balls of sponge or so many foot-balls. Give to those what compressibility you please, common air could no more be fluid than a mass of clay; it could no more be fluid than a mass of such balls pressed into a box. It can be demonstrated (and indeed hardly needs a demonstration), that before a parcel of such balls, just touching each other, can be squeezed into half their present dimensions, their globular shape will be entirely gone, and each will have become a perfect cube, touching six other cubes with its whole surface; and these cubes will be strongly compressed together, so that motion could never be performed through among them by any solid body without a very great force. Whereas we know that in this state air is just as permeable to every body as the common air that we breathe. There is no way in which we can represent this fluidity to our imagination but by conceiving air to consist of particles, not only discrete, but distant from each other, and actuated by repulsive forces, or something analogous to them. It is an idle subterfuge, to which some naturalists have recourse, saying, that they are kept asunder by an intervening ether, or elastic fluid of any other name. This is only removing the difficulty a step farther off; for the elasticity of this fluidity requires the same explanation; and therefore it is necessary, in obedience to the rules of just reasoning, to begin the inquiry here; that is, to determine from the phenomena what is the analogy between the distances of the particles and the repulsive forces exerted at these distances, proceeding in the same way as in the examination of planetary gravitation. We shall learn the analogy by attending to the analogy between the compressing force and the density.

For the density depends on the distance between the particles; the nearer they are to each other, the denser is the air. Suppose a square pipe one inch wide and eight inches long, shut at one end, and filled with common air; then suppose a plug so nicely fitted to this pipe that no air can pass by its sides; suppose this piston thrust down to within an inch of the bottom; it is evident that the air which formerly filled the whole pipe now occupies the space of one cubic inch, which contains the same number of particles as were formerly diffused over eight cubic inches.

The condensation would have been the same if the air which fills a cube whose side is two inches had been squeezed into a cube of one inch, for the cube of two inches also contains eight inches. Now, in this case it is evident that the distance between the particles would be reduced to its half in every direction. In like manner, if a cube whose side is three inches, and which therefore contains 27 inches, be squeezed into one inch, the distance of the particles will be one third of what it was: in general the distance of the particles will be as the cube-root of the space into which they are compressed. If the space be $\frac{1}{8}$, $\frac{1}{27}$, $\frac{1}{64}$, $\frac{1}{125}$, &c. of its former dimensions, the distance of the particles will be $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. Now the term *density*, in its strict sense, expresses the vicinity of the particles; *densi arbores* are trees growing near each other. The measure of this vicinity therefore is the true measure of the density; and when 27 inches of air are compressed into one, we should say that it is three times as dense; but we say, that it is 27 times denser.

Density is therefore used in a sense different from its strictest acceptation: it expresses the comparative number of equidistant particles contained in the same bulk. This is also abundantly precise, when we compare bodies of the same kind, differing in density only; but we also say, that gold is 19 times denser than water, because the same bulk of it is 19 times heavier. This assertion proceeds on the assumption, or the fact, that every ultimate atom of terrestrial matter is equally heavy; a particle of gold may contain more or fewer atoms of matter than a particle of water. In such a case, therefore, the term density has little or no reference to the vicinity of the particles; and is only a term of comparison of other qualities or accidents.

But when we speak of the respective densities of the same substance in its different states of compression, the word *density* is strictly connected with vicinity of particles, and we may safely take either of the measures. We shall abide by the common acceptation, and call that air eight times as dense which has eight times as many particles in the same bulk, although the particles are only twice as near to each other.

Thus then we see, that by observing the analogy between the compressing force and the density, we shall discover the analogy between the compressing force and the distance of the particles. Now the force which is necessary for compressing two particles of air to a certain vicinity is a proper measure of the elasticity of the particles corresponding to that vicinity or distance; for it balances it, and forces which balance must be esteemed equal. Elasticity is a distinctive name for that corpuscular force which keeps the particles at that distance; therefore observations made on the analogy between the compressing force and the density of air will give us the law of its corpuscular force, in the same way that observations on the simultaneous deflections of the planets towards the sun give us the law of celestial gravitation.

But the sensible compressing forces which we are able to apply is at once exerted on unknown thousands of particles, while it is the law of action of a single particle that we want to discover. We must therefore know the *proportion* of the numbers of particles on which the compressing force is exerted. It is easy to see, that since the distance of the particles is as the cube root of the density inversely, the number of particles in physical contact with the compressing surface must be as the square of this root. Thus when a cube of eight inches is compressed into one inch, and the particles are twice as near each other as they were before, there must be four times the number of particles in contact with each of the sides of this cubical inch; or, when we have pushed down the square piston of the pipe spoken of above to within an inch of the bottom, there will be four times the number of particles *immediately* contiguous to the piston, and resisting the compression; and in order to obtain the force really exerted on one particle, and the elasticity of that particle, we must divide the whole compressing force by 4. In like manner, if we have compressed air into $\frac{1}{27}$ of its former bulk, and brought the particles to $\frac{1}{3}$ of their former distance, we must divide the compressing force by 9. In general, if d express the density, $\frac{1}{\sqrt[3]{d}}$ will express the distance x of the particles; $\sqrt[3]{d}$, or $d^{\frac{1}{3}}$, will express the vicinity or real density; and $d^{\frac{2}{3}}$ will express the number of particles acting on the compressing surface: and if f express the accumulated external compressing force, $\frac{f}{d^{\frac{2}{3}}}$ will express the force acting on one particle; and therefore the elasticity of that particle corresponding to the distance x.

We may now proceed to consider the experiments by which the law of compression is to be established.

The first experiments to this purpose were those made by Mr Boyle, published in 1661, in his *Defensio Doctrinæ de Aeris Elatere contra Linum*, and exhibited before the Royal Society the year before. Mariotte made experiments of the same kind, which were published in 1676, in his *Essais ur la Nature de l'Air* and *Traité des Mouvemens des Eaux*. The most copious experiments are those by Sulzer

(*Mem. Berlin*, ix.), those by Fontana (*Opusc. Physico-Math.*), and those by Shuckburgh and Gen. Roy.

In order to examine the compressibility of air that is not rarer than the atmosphere at the surface of the earth, we employ a bent tube or cyphon ABCD (Fig. 66.), hermetically sealed at A and open at D. The short leg AB must be very accurately divided in the proportion of its solid contents, and fitted with a scale whose units denote equal increments, not of length, but of capacity. There are various ways of doing this; but it requires the most scrupulous attention, and without this the experiments are of no value. In particular, the arched form at A must be noticed. A small quantity of mercury must then be poured into the tube, and passed backwards and forwards till it stands (the tube being held in a vertical position) on a level at B and C. Then we are certain that the included air is of the same density with that of the contiguous atmosphere. Mercury is now poured into the leg DC, which will fill it, suppose to G, and will compress the air into a smaller space AE. Draw the horizontal line EF: the new bulk of the compressed air is evidently AE, measured by the adjacent scale, and the addition made to the compressing force of the atmosphere is the weight of the column GF. Produce GF downwards to H, till FH is equal to the height shown by a Toricellian tube filled with the *same mercury*; then the whole compressing force is HG. This is evidently the measure of the elasticity of the compressed air in AE, for it balances it. Now pour in more mercury, and let it rise to g, compressing the air into A e. Draw the horizontal line ef, and make fh equal to FH; then A e will be the new bulk of the compressed air, $\frac{AB}{Ae}$ will be its new density, and hg will be the measure of the new elasticity. This operation may be extended as far as we please, by lengthening the tube CD, and taking care that it be strong enough to resist the great pressure. Great care must be taken to keep the whole in a constant temperature, because the elasticity of air is greatly affected by heat, and the change by any increase of temperature is different according to its density or compression.

The experiments of Boyle, Mariotte, Amontons, and others, were not extended to very great compressions, the density of the air not having been quadrupled in any of them; nor do they seem to have been made with very great nicety. It may be collected from them in general, that the elasticity of the air is very nearly proportioned to its density; and accordingly this law was almost immediately acquiesced in, and was called the *Boylean law*: it is accordingly assumed by almost all writers on the subject as exact. Of late years, however, there occurred questions in which it was of importance that this point should be more scrupulously settled, and the former experiments were repeated and extended. Sulzer and Fontana have carried them farther than any other. Sulzer compressed air into $\frac{1}{8}$ of its former dimensions.

Considerable varieties and irregularities are to be observed in these experiments. It is extremely difficult to preserve the temperature of the apparatus, particularly of the leg AB, which is most handled. A great quantity of mercury must be employed; and it does not appear that philosophers have been careful to have it precisely similar to that in the barometer, which gives us the unit of compressing force, and of elasticity. The mercury in the barometer should be pure and boiled. If the mercury in the syphon is adulterated with bismuth and tin, which it commonly is to a considerable degree, the compressing force, and consequently the elasticity, will appear greater than the truth. If the barometer has not been nicely fitted, it will be lower than it should be, and the compressing force will appear too great, because the unit is too small; and this error will be most remarkable in the smaller compressions.

The greatest source of error and irregularity in the experiments is the very heterogeneous nature of the air itself. Air is a solvent of all fluids, all vapours, and perhaps of many solid bodies. It is highly improbable that the different compounds shall have the same elasticity, or even the same law of elasticity: and it is well known, that air, loaded with water or other volatile bodies, is much more expansible by heat than pure air; nay, it would appear from many experiments, that certain determinate changes both of density and of temperature, cause air to let go the vapours which it holds in solution. Cold causes it to precipitate water, as appears in dew; so does rarefaction, as is seen in the receiver of an air-pump.

In general, it appears that the elasticity of air does not increase quite so fast as its density. This will be best seen by the following tables, calculated from the experiments of M. Sulzer. The column E in each set of experiments expresses the length of the column GH, the unit being FH, while the column D expresses $\frac{AB}{AE}$.

First Set.		Second Set.		Third Set.	
D	E	D	E	D	E
1,000	1,000	1,000	1,000	1,000	1,000
1,100	1,093	1,236	1,224	1,091	1,076
1,222	1,211	1,294	1,288	1,200	1,183
1,375	1,284	1,375	1,332	1,333	1,303
1,571	1,559	1,466	1,417	1,500	1,472
1,692	1,669	1,571	1,515	1,714	1,659
1,833	1,796	1,692	1,647		
2,000	1,958	2,000	1,964	2,000	1,900
2,288	2,130				
2,444	2,375	2,444	2,392	2,400	2,241
3,143	2,936	3,143	3,078	3,000	2,793
3,666	3,391	3,666	3,575		
4,000	3,706			4,000	3,631
4,444	4,035	4,444	4,320		
4,888	4,438				
5,500	4,922	5,500	5,096		
5,882	5,522			6,000	5,297
		7,333	6,694		
				8,000	6,836

There appears in these experiments sufficient grounds for calling in question the Boylean law; and the writer of this article thought it incumbent on him to repeat them with some precautions, which probably had not been attended to by Mr Sulzer. He was particularly anxious to have the air as free as possible from moisture. For this purpose, having detached the short leg of the syphon, which was 34 inches long, he boiled mercury in it, and filled it with mercury boiling hot. He took a tinplate vessel of sufficient capacity, and put into it a quantity of powdered quicklime just taken from the kiln; and having closed the mouth, he agitated the lime through the air in the vessel, and allowed it to remain there all night. He then emptied the mercury out of the syphon into this vessel, keeping the open end far within it. By this means the short leg of the syphon was filled with very dry air. The other part was now joined, and boiled mercury put into the bend of the syphon; and the experiment was then prosecuted with mercury which had been recently boiled, and was the same with which the barometer had been carefully filled.

The results of the experiments are expressed in the following table.

Dry Air.		Moist Air.		Damp Air.	
D	E	D	E	D	E
1,000	1,000	1,000	1,000	1,000	1,000
2,000	1,957	2,000	1,920	2,000	1,909
3,000	2,848	3,000	2,839	3,000	2,845
4,000	3,737	4,000	3,726	4,000	3,718
5,500	4,930	5,500	5,000	5,500	5,104
6,000	5,342	6,000	5,452	6,000	5,463
7,620	6,490	7,620	6,775	7,620	6,812

Here it appears again in the clearest manner, that the elasticities do not increase as fast as the densities, and the differences are even greater than in Mr Sulzer's experiments.

The second table contains the results of experiments made on very damp air in a warm summer's morning. In these it appears that the elasticities are almost precisely proportional to the densities + a small constant quantity, nearly 0,11 deviating from this rule chiefly between the densities 1 and 1,5, within which limits we have very nearly $D = E^{1.0017}$. As this air is nearer to the constitution of atmospheric air than the former, this rule may be safely followed in cases where atmospheric air is concerned, as in measuring the depths of pits by the barometer.

The third table shows the compression and elasticity of air strongly impregnated with the vapours of camphire. Here the Boylean law appears pretty exact, or rather the elasticity seems to increase a little faster than the density.

Dr Hooke examined the compression of air by immersing a bottle to great depths in the sea, and weighing the water which got into it without any escape of air. But this method was liable to great uncertainty, on account of the unknown temperature of the sea at great depths.

Hitherto we have considered only such air as is not rarer than what we breathe; we must take a very different method for examining the elasticity of rarefied air.

Let *g h* (Fig. 67.) be a long tube, formed a-top into a cup, and of sufficient diameter to receive another smaller tube *a f*, open at first at both ends. Let the outer tube and cup be filled with mercury, which will rise in the inner tube to the same level. Let *a f* now be stopped at *a*. It contains air of the same density and elasticity with the adjoining atmosphere. Note exactly the space *a b* which it occupies. Draw it up into the position of Fig. 68. and let the mercury stand in it at the height *d e*, while *c e* is the height of the mercury in the barometer. It is evident that the column *d e* is in equilibrio between the pressure of the atmosphere and the elasticity of the air included in the space *a d*. And since the weight of *c e* would be in equilibrio with the whole pressure of the atmosphere, the weight of $c\,d$ is equivalent to the elasticity of the included air. While therefore $c\,e$ is the measure of the elasticity of the surrounding atmosphere, $c\,d$ will be the measure of the elasticity of the included air; and since the air originally occupied the space $a\,b$, and has now expanded into $a\,d$, we have $\frac{a\,b}{a\,d}$ for the measure of its density. N.B. $c\,e$ and $c\,d$ are measured by the *perpendicular heights* of the columns, but $a\,b$ and $a\,d$ must be measured by their *solid capacities*.

By raising the inner tube still higher, the mercury will also rise higher, and the included air will expand still farther, and we obtain another $c\,d$, and another $\frac{a\,b}{a\,d}$; and in this manner the relation between the density and elasticity of rarefied air may be discovered.

This examination may be managed more easily by means of the air-pump. Suppose a tube $a\,e$ (Fig. 69.) containing a small quantity of air $a\,b$, set up in a cistern of mercury, which is supported in the tube at the height $e\,b$, and let $e\,c$ be the height of the mercury in the barometer. Let this apparatus be set under a tubulated receiver on the pump-plate, and let $g\,n$ be the pump-gage, and $m\,n$ be made equal to $c\,e$.

Then, as has been already shewn, $c\,b$ is the measure of the elasticity of the air in $a\,b$, corresponding to the bulk $a\,b$. Now let some air be abstracted from the receiver. The elasticity of the remainder will be diminished by its expansion; and therefore the mercury in the tube $a\,e$ will descend to some point d. For the same reason, the mercury in the gage will rise to some point o, and $m\,o$ will express the elasticity of the air in the receiver. This would support the mercury in the tube $a\,e$ the height $e\,r$, if the space $a\,r$ were entirely void of air. Therefore $r\,d$ is the effect and measure of the elasticity of the included air when it has expended to the bulk $a\,d$; and thus its elasticity, under a variety of other bulks, may be compared with its elasticity when of the bulk *a b*. When the air has been so far abstracted from the receiver that the mercury in *a e* descends to *e*, then *m o* will be the precise measure of its elasticity.

In all these cases it is necessary to compare its bulk *a b* with its natural bulk, in which its elasticity balances the pressure of the atmosphere. This may be done by laying the tube *a e* horizontally, and then the air will collapse into its ordinary bulk.

Another easy method may be taken for this examination. Let an apparatus *a b c d e f* (Fig. 70.) be made, consisting of a horizontal tube *a e* of even bore, a ball *d g e* of a large diameter, and a swan-neck tube *h f*. Let the ball and part of the tube *g e b* be filled with mercury, so that the tube may be in the same horizontal plane with the surface *d e* of the mercury in the ball. Then seal up the end *a*, and connect *f* with an air-pump. When the air is abstracted from the surface *d e*, the air in *a b* will expand into a larger bulk *a c*, and the mercury in the pump-gage will rise to some distance below the barometric height. It is evident that this distance, without any farther calculation, will be the measure of the elasticity of the air pressing on the surface *d e*, and therefore of the air in *a c*.

The most exact of all methods is to suspend in the receiver of an air-pump a glass vessel, having a very narrow mouth over a cistern of mercury, and then abstract the air till the gage rises to some determined height. The difference *e* between this height and the barometric height determines the elasticity of the air in the receiver and in the suspended vessel. Now lower down that vessel by the slip-wire till its mouth is immersed into the mercury, and admit the air into the receiver; it will press the mercury into the little vessel. Lower it still farther down, till the mercury within it is level with that without; then stop its mouth, take it out and weigh the mercury, and let its weight be w. Subtract this weight from the weight v of the mercury, which would completely fill the whole vessel; then the natural bulk of the air will be $v-w$, while its bulk, when of the elasticity e in the rarefied receiver, was the bulk or capacity w of the vessel. Its density, therefore, corresponding to this elasticity e, was $\frac{v-w}{w}$. And thus may the relation between the density and elasticity in all cases be obtained.

A great variety of experiments to this purpose have been made, with different degrees of attention, according to the interest which the philosophers had in the result. Those made by De Luc, General Roy, Trembley, and Shuckburgh, are by far the most accurate; but they are all confined to very moderate rarefactions. The general result has been, that the elasticity of rarefied air is very nearly proportional to its density. We cannot say with confidence that any regular deviation from this law has been observed, there being as many observations on one side as on the other; but we think that it is not unworthy the attention of philosophers to determine it with precision in the cases of extreme rarefaction, where the irregularities are most remarkable. The great source of error is a certain adhesive sluggishness of the mercury when the impelling forces are very small; and other fluids can hardly be used, because they either smear the inside of the tube and diminish its capacity, or they are converted into vapour, which alters the law of elasticity.

Let us, upon the whole, assume the Boylean law, *viz.* that the elasticity of the air is proportional to its density. The law deviates not in any sensible degree from the truth in those cases which are of the greatest practical importance, that is, when the density does not much exceed or fall short of that of ordinary air.

Let us now see what information this gives us with respect to the action of the particles on each other.

The investigation is extremely easy. We have seen that a force eight times greater than the pressure of the atmosphere will compress common air into the eighth part of its common bulk, and give it eight times its common density: and in this case we know, that the particles are at half their former distance, and that the number which are now acting on the surface of the piston employed to compress them is quadruple of the number which act on it when it is of the common density. Therefore, when this eightfold compressing force is distributed over a fourfold number of particles, the portion of it which acts on each is double. In like manner, when a compressing force 27 is employed, the air is compressed into $\frac{1}{27}$ of its former bulk, the particles are at $\frac{1}{3}$ of their former distance, and the force is distributed among 9 times the number of particles; the force on each is therefore 3. In short, let $\frac{1}{x}$ be the distance of the particles, the number of them in any given vessel, and therefore the density will be as x^3, and the number pressing by their elasticity on its whole internal surface will be as x^2. Experiment shows, that the compressing force is as x^3, which being distributed over the number as x^2, will give the force on each as x. Now this force is in immediate equilibrium with the elasticity of the particle immediately contiguous to the compressing surface. This elasticity is therefore as x: and it follows from the nature of perfect fluidity, that the particle adjoining to the compressing surface presses with an equal force on its adjoining particles on every side. Hence we must conclude, that the corpuscular repulsions exerted by the adjoining particles are inversely as their distances from each other, or that the adjoining particles tend to recede from each other with forces inversely proportional to their distances.

Sir Isaac Newton was the first who reasoned in this manner from the phenomena. Indeed he was the first who had the patience to reflect on the phenomena with any precision. His discoveries in gravitation naturally gave his thoughts this turn, and he very early hinted his suspicions that all the charateristic phenomena of tangible matter were produced by forces which were exerted by the particles at small and insensible distances. And he considers the phenomena of air as affording an excellent example of this investigation, and deduces from them the law which we have now demonstrated; and says, that air consists of particles which avoid the adjoining particles with forces inversely proportional to their distances from each other. From this he deduces (in the 2d book of his Principles) several beautiful propositions, determining the mechanical constitution of the atmosphere.

But it must be noticed that he limits this action to the *adjoining* particles: and this is a remark of immense consequence, though not attended to by the numerous experimenters who adopt the law.

It is plain that the particles are supposed to act at a distance, and that this distance is variable, and that the forces diminish as the distances increase. A very ordinary air-pump will rarefy the air 125 times. The distance of the particles is now five times greater than before; and yet they still repel each other: for air of this density will still support the mercury in a syphon-gage at the height of 0,24, or $\frac{24}{100}$ of an inch; and a better pump will allow this air to expand twice as much, and still leave it elastic. Thus we see that whatever is the distance of the particles of common air, they can act five times farther off. The question comes now to be, Whether in the state of common air, they really do act five times farther than the distance of the adjoining particles? While the particle *a* acts on the particle *b* with the force 5, does it also act on the particle *c* with the force 2,5, on the particle *d* with the force 1,667, on the particle *e* with the force 1,25, on the particle *f* with the force 1, on the particle *g* with the force 0,8333, &c.?

Sir Isaac Newton shows in the plainest manner, that this is by no means the case; for if this were the case, he makes it appear that the sensible phenomena of condensation would be totally different from what we observe. The force necessary for a quadruple condensation would be eight times greater, and for a nonuple condensation the force must be 27 times greater. Two spheres filled with condensed air must repel each other, and two spheres containing air that is rarer than the surrounding air must attract each other, &c. &c. All this will appear very clearly, by applying to air the reasoning which Sir Isaac Newton has employed in deducing the sensible law of mutual tendency of two spheres, which consist of particles attracting each other with forces proportional to the square of the distance inversely.

If we could suppose that the particles of air repelled each other with invariable forces at all distances within some small and insensible limit, this would produce a compressibility and elasticity similar to what we observe. For if we consider a row of particles, within this limit, as compressed by an external force applied to the two extremities, the action of the whole row on the extreme points would be proportional to the number of particles, that is, to their distance inversely and to their density: and a number of such parcels, ranged in a straight line, would constitute a row of any sensible magnitude having the same law of compression. But this law of corpuscular force is unlike every thing we observe in nature, and to the last degree improbable.

We must therefore continue the limitation of this mutual repulsion of the particles of air, and be contented for the present with having established it as an experimental fact, that the *adjoining* particles of air are kept asunder by forces inversely proportional to their distances; or perhaps it is better to abide by the sensible law, that *the density of air is proportional to the compressing force*. This law is abundantly sufficient for explaining all the subordinate phenomena, and for giving us a complete knowledge of the mechanical constitution of our atmosphere.

And, in the *first* place, this view of the compressibility of the air must give us a very different notion of the height of the atmosphere from what we deduced on a former occasion from our experiments. It is found, that when the air is of the temperature 32° of Fahrenheit's thermometer, and the mercury in the barometer stands at 30 inches, it will descend one-tenth of an inch if we take it to a place 87 feet higher. Therefore, if the air were equally dense and heavy throughout, the height of the atmosphere would be 30 × 10 × 87 feet, or five miles and 100 yards. But the loose reasoning adduced on that occasion was enough to show us that it must be much higher; because every stratum as we ascend must be successively rarer as it is less compressed by incumbent weight. Not knowing to what degree air expanded when the compression was diminished, we could not tell the successive diminutions of density and consequent augmentation of bulk and height; we could only say, that several atmospheric appearances indicated a much greater height. Clouds have been seen much higher; but the phenomenon of the twilight is the most convincing proof of this. There is no doubt that the visibility of the sky or air is owing to its want of perfect transparency, each particle (whether of matter purely aerial or heterogeneous) reflecting a little light.

Let *b* (Fig. 71.) be the last particle of illuminated air which can be seen in the horizon by a spectator at A. This must be illuminated by a ray S D *b*, touching the earth's surface at some point D. Now it is a known fact, that the degree of illumination called *twilight* is perceived when the sun is 18° below the horizon of the spectator, that is, when the angle E *b* S or A C D is 18 degrees; therefore *b* C is the secant of nine degrees; (it is less, viz. about 8½ degrees, on account of refraction). We know the earth's radius to be about 3910 miles: hence we conclude *b* B to be about 45 miles; nay, a very sensible illumination is perceptible much farther from the sun's place than this, perhaps twice as far, and the air is sufficiently dense for reflecting a sensible light at the height of nearly 900 miles.

We have now seen that air is prodigiously expansible. None of our experiments have distinctly shown us any limit. But it does not follow that it is expansible without end; nor is this at all likely. It is much more probable that there is a certain distance of the parts in which they no longer repel each other; and this would be the distance at which they would arrange themselves if they were not heavy. But at the very summit of the atmosphere they will be a very small matter nearer to each other, on account of their gravitation to the earth. Till we know precisely the law of this mutual repulsion, we cannot say what is the height of the atmosphere.

But if the air be an elastic fluid whose density is always proportionable to the compressing force, we can tell what is its density at any height above the surface of the earth: and we can compare the density so calculated with the density discovered by observation: for this last is measured by the height at which it supports mercury in the barometer. This is the direct measure of the pressure of the external air; and as we know the law of gravitation, we can tell what would be the pressure of air having the calculated density in all its parts.

Let us therefore suppose a prismatic or cylindric column of air reaching to the top of the atmosphere. Let this be divided into an indefinite number of strata of very small and equal depths or thickness; and let us, for greater simplicity, suppose at first that a particle of air is of the same weight at all distances from the centre of the earth.

The absolute weight of any one of these strata will on these conditions be proportional to the number of particles, or the gravity of air contained in it; and since the depth of each stratum is the same, this quantity of air will evidently be as the density of the stratum: but the density of any stratum is as the compressing force; that is, as the pressure of the strata above it; that is, as their weight; that is, as their quantity of matter—therefore the quantity of air in each stratum is proportional to the quantity of air above it; but the quantity in each stratum is the difference between the column incumbent on its bottom and on its top: these differences are therefore proportional to the quantities of which they are the differences. But when there is a series of quantities which are proportional to their own differences, both the quantities and their differences are in continual or geometrical progression: for let a, b, c, be three such quantities that

$b : c = a - b : b - c$, then, by altern.
$b : a - b = c : b - c$ and by compos.
$b : a = c : b$
and $a : b = b : c$

therefore the densities of these strata decrease in a geometrical progression; that is, when the elevations above the centre or surface of the earth increase, or their depths under the top of the atmosphere decrease, in an arithmetical progression, the densities decrease in a geometrical progression.

Let A R Q (Fig. 72.) represent the section of the earth by a plane through its centre O, and let m O A M be a vertical line, and A E perpendicular to O A will be a horizontal line through A, a point on the earth's surface. Let A E be taken to represent the density of the air at A; and let D H, parallel to A E, be taken to A E as the density at D is to the density at A: it is evident, that if a logistic or logarithmic curve E H N be drawn, having A N for its axis, and passing through the points E and H, the density of the air at any other point C, in this vertical line, will be represented by C G, the ordinate to the curve in that point: for it is the property of this curve, that if portions A B, A C, A D, of its axis be taken in arithmetical progression, the ordinates A E, B F, C G, D H, will be in geometrical progression.

It is another fundamental property of this curve, that if E K or H S touch the curve in E or H, the subtangent A K or D S is a constant quantity.

And a third fundamental property is, that the infinitely extended area MAEN is equal to the rectangle KAEL of the ordinate and subtangent; and, in like manner, the area MDHN is equal to SD × DH, or to KA × DH; consequently the area lying beyond any ordinate is proportional to that ordinate.

These geometrical properties of this curve are all analogous to the chief circumstances in the constitution of the atmosphere, on the supposition of equal gravity. The area MCGN represents the whole quantity of aereal matter which is above C: for CG is the density at C, and CD is the thickness of the stratum between C and D; and therefore CGHD will be as the quantity of matter or air in it; and in like manner of all the others, and of their sums, or the whole area MCGN: and as each ordinate is proportional to the area above it, so each density, and the quantity of air in each stratum, is proportional to the quantity of air above it: and as the whole area MAEN is equal to the rectangle KAEL, so the whole air of variable density above A might be contained in a column KA, if, instead of being compressed by its own weight, it were without weight, and compressed by an external force equal to the pressure of the air at the surface of the earth. In this case, it would be of the uniform density AE, which it has at the surface of the earth, making what we have repeatedly called the homogeneous atmosphere.

Hence we derive this important circumstance, that the height of the homogeneous atmosphere is the subtangent of that curve whose ordinates are as the densities of the air at different heights, on the supposition of equal gravity. This curve may with propriety be called the ATMOSPHERICAL LOGARITHMIC: and as the different logarithmics are all characterized by their subtangents, it is of importance to determine this one.

It may be done by comparing the densities of mercury and air. For a column of air of uniform density, reaching to the top of the homogeneous atmosphere, is in equilibrio with the mercury in the barometer. Now it is found by the best experiments, that when mercury and air are of the temperature 32° of Fahrenheit's thermometer, and the barometer stands at 30 inches, the mercury is nearly 10440 times denser than air. Therefore the height of the homogeneous atmosphere is 10440 times 30 inches, or 26100 feet, or 8700 yards, or 4350 fathoms, or five miles wanting 100 yards.

Or it may be found by observations on the barometer. It is found, that when the mercury and air are of the above temperature, and the barometer on the sea-shore stands at 30 inches, if we carry it to a place 884 feet higher it will fall to 29 inches. Now, in all logarithmic curves having equal ordinates, the portions of the axis intercepted between the corresponding pairs of ordinates are proportional to the subtangents. And the subtangents of the curve belonging to our common tables is 0,4342945, and the difference of the logarithms of 30 and 29 (which is the portion of the axis intercepted between the ordinates 30 and 29), or 0,0147233, is to 0,4342945 as 883 is to 26058 feet, or 8686 yards, or 4343 fathoms, or 5 miles wanting 114 yards. This determination is 14 yards less than the other, and it is uncertain which is the most exact. It is extremely difficult to measure the respective densities of mercury and air; and in measuring the elevation which produces a fall of one inch in the barometer, an error of $\frac{1}{20}$ of an inch would produce all the difference. We prefer the last, as depending on fewer circumstances.

But all this investigation proceeds on the supposition of equal gravity, whereas we know that the weight of a particle of air decreases as the square of its distance from the centre of the earth increases. In order, therefore, that a superior stratum may produce an equal pressure at the surface of the earth, it must be denser, because a particle of it gravitates less. The density, therefore, at equal elevations, must be greater than on the supposition of equal gravity, and the law of diminution of density must be different.

Make $OD : OA = OA : Od$.
$OC : OA = OA : Oc$;
$OB : OA = OA : Ob$, &c.

so that Od, Oc, Ob, OA, may be reciprocals to OD, OC, OB, OA; and through the points A, *b*, *c*, *d*, draw the perpendiculars AE, *bf*, *cg*, *d b*, making them proportional to the densities in A, B, C, D: and let us suppose CD to be exceedingly small, so that the density may be supposed uniform through the whole stratum. Thus we have

$$OD \times Od = OA^2, = OC \times Oc$$
and $Oc : Od = OD : OC$;
and $Oc : Oc - Od = OD : OD - OC$,
or $Oc : cd = OD : DC$;
and $cd : CD = Oc : OD$;

or, because OC and OD are ultimately in the ratio of equality, we have

$$cd : CD = Oc : OC = OA^2 : OC^2,$$

and $cd = CD \times \frac{OA^2}{OC^2}$, and $cd \times cg = CD \times cg \times \frac{OA^2}{OC^2}$;

but $CD \times cg \times \frac{OA^2}{OC^2}$ is as the pressure at C arising from the absolute weight of the stratum CD. For this weight is as the bulk, as the density, and as the gravitation of each particle jointly. Now CD expresses the bulk, *cg* the density, and $\frac{OA^2}{OC^2}$ the gravitation of each particle. Therefore, $cd \times cg$ is as the pressure on C arising from the weight of the stratum DC; but $cd \times cg$ is evidently the element of the curvilineal area A$mn$E, formed by the curve E$fghn$ and the ordinates AE, bf, cg, ah, &c. mn. Therefore the sum of all the elements, such as $cdhg$, that is, the area $cmng$ below cg, will be as the *whole* pressure on C, arising from the gravitation of all the air above it; but, by the nature of air, this whole pressure is as the density which it produces, that is, as cg. Therefore the curve Egn is of such a nature that the area lying below or beyond any ordinate cg is proportional to that ordinate. This is the property of the logarithmic curve, and Egn is a logarithmic curve.

But farther, this curve is the same with EGN. For let B continually approach to A, and ultimately coincide with it. It is evident that the ultimate ratio of BA to Ab, and of BF to bf, is that of equality; and if EFK, Efk, be drawn, they will contain equal angles with the ordinate AE, and will cut off equal subtangents AK, Ak. The curves EGN, Egn are therefore the same, but in opposite positions.

Lastly, if OA, Ob, Oc, Od, &c. be taken in arithmetical progression decreasing, their reciprocals OA, OB, OC, OD, &c. will be in harmonical progression increasing, as is well known: but, from the nature of the logarithmic curve, when OA, Ob, Oc, Od, &c. are in arithmetical progression, the ordinates AE, bf, cg, dh, &c. are in geometrical progression. Therefore when OA, OB, OC, OD, &c. are in harmonical progression, the densities of the air at A, B, C, D, &c. are in geometrical progression; and thus may the density of the air at all elevations be discovered. Thus to find the density of the air at K the top of the homogeneous atmosphere, make OK : OA = OA : OL, and draw the ordinate LT, LT is the density at K.

The celebrated Dr Halley was the first who observed the relation between the density of the air and the ordinates of the logarithmic curve, or common logarithms. This he did on the supposition of equal gravity; and his discovery is acknowledged by Sir Isaac Newton, in *Princip.* ii. *prop.* 22. *schol.* Halley's dissertation on the subject is in No 185. of the Phil. Trans. Newton, with his usual sagacity, extended the same relation to the true state of the case, where gravity is as the square of the distance inversely; and showed that when the distances from the earth's centre are in harmonic progression, the densities are in geometric progression. He shows indeed, in general, what progression of the distance, on any supposition of gravity, will produce a geometrical progression of the densities, so as to obtain a set of lines OA, O *b*, O *c*, O *d*, &c. which will be logarithms of the densities. The subject was afterwards treated in a more familiar manner by Cotes in his *Hydrost. Lect.* and in his *Harmonia Mensurarum*; also by Dr Brooke Taylor, *Meth. Increment*; Wolf in his *Aërometria*; Herman in his *Phoronomia*; &c. &c. and lately by Horsley, Phil. Trans. tom. lxiv.

We have shown how to determine *à priori* the density of the air at different elevations above the surface of the earth. But the densities may be discovered in all accessible elevations by experiments; namely, by observing the heights of the mercury in the barometer. This is a direct measure of the pressure of the incumbent atmosphere; and this is proportional to the density which it produces.

Therefore, by means of the relation subsisting between the densities and the elevations, we can discover the elevations by observations made on the densities by means of the barometer; and thus we may measure elevations by means of the barometer; and, with very little trouble, take the level of any extensive tract of country. Of this we have an illustrious example in the section which the Abbé Chappe D'Auteroche has given of the whole country between Brest and Ekaterinenburgh in Siberia. This is a subject which deserves a minute consideration; we shall therefore present it under a very simple and familiar form; and trace the method through its various steps of improvement by De Luc, Roy, Shuckburgh, &c.

We have already observed oftener than once, that if the mercury in the barometer stands at 30 inches, and if the air and mercury be of the temperature 32° in Fahrenheit's thermometer, a column of air 87 feet thick has the same weight with a column of mercury $\frac{1}{10}$ of an inch thick. Therefore, if we carry the barometer to a higher place, so that the mercury sinks to 29.9, we have ascended 87 feet. Now, suppose we carry it still higher, and that the mercury stands at 29.8; it is required to know what height we have now got to? We have evidently ascended through another stratum of equal weight with the former: but it must be of greater thickness, because the air in it is rarer, being less compressed. We may call the density of the first stratum 300, measuring the density by the number of tenths of an inch of mercury which its elasticity proportional to its density enables it to support. For the same reason, the density of the second stratum must be 299; but when the weights are equal, the bulks are inversely as the densities; and when the bases of the strata are equal, the bulks are as the thicknesses. Therefore, to obtain the thickness of this second stratum, say 299:300 = 87:87,29; and this fourth term is the thickness of the second stratum, and we have ascended in all 174,29 feet. In like manner we may rise till the barometer shows the density to be 298: then say, 298 : 30 = 87 : 87,584 for the thickness of the third stratum, and 261,875 or $261\frac{7}{8}$ for the whole ascent; and we may proceed in the same way for any number of mercurial heights, and make a table of the corresponding elements as follows: where the first column is the height of the mercury in the barometer, the second column is the thickness of the stratum, or the elevation above the preceding station; and the third column is the whole elevation above the first station.

Bar.	Strat.	Elev.
30	00,000	00,000
29,9	87,000	87,000
29,8	87,291	174,291
29,7	87,584	261,875
29,6	87,879	349,754
29,5	88,176	437,930
29,4	88,475	526,405
29,3	88,776	615,181
29,2	89,079	704,260
29,1	89,384	793,644
29	89,691	883,335

Having done this, we can now measure any elevation within the limits of our table, in this manner:

Observe the barometer at the lower and at the upper stations, and write down the corresponding elevations. Subtract the one from the other, and the remainder is the height required. Thus suppose that at the lower station the mercurial height was 29,8, and that at the upper station it was 29,1.

29,1	793,644
29,8	174,291
	619,353 = Elevation.

We may do the same thing with tolerable accuracy without the table, by taking the medium m of the mercurial heights, and their difference d in tenths of an inch; and then say, as m to 300, so is $87d$ to the height required h; or $h = \frac{300 \times 87d}{m} = \frac{26100d}{m}$. Thus, in the foregoing example, m is 294,5, and d is $= 7$; and therefore $h = \frac{7 \times 26100}{294,5} = 620,4$, differing only one foot from the former value.

Either of these methods is sufficiently accurate for most purposes, and even in very great elevations will not produce any error of consequence: the whole error of the elevation 883 feet 4 inches, which is the extent of the above table, is only $\frac{3}{4}$ of an inch.

But we need not confine ourselves to methods of approximation, when we have an accurate and scientific method that is equally easy. We have seen that, upon the supposition of equal gravity, the densities of the air are as the ordinates of a logarithmic curve, having the line of elevations for its axis. We have also seen that, in the true theory of gravity, if the distances from the centre of the earth increase in a harmonic progression, the logarithm of the densities will decrease in an arithmetical progression; but if the greatest elevation above the surface be but a few miles, this harmonic progression will hardly differ from an arithmetical one. Thus, if A*b*, A*c*, A*d*, are 1, 2, and 3 miles, we shall find that the corresponding elevations AB, AC, AD are sensibly in arithmetical progression also: for the earth's radius AC is nearly 4000 miles. Hence it plainly follows, that BC—AB is $\frac{1}{4000 \times 4001}$, or $\frac{1}{16004000}$ of a mile, or $\frac{1}{250}$ of an inch; a quantity quite insignificant. We may therefore affirm without hesitation, that in all accessible places, the elevations increase in an arithmetical progression, while the densities decrease in a geometrical progression. Therefore the ordinates are proportional to the numbers which are taken to measure the densities, and the portions of the axis are proportional to the logarithms of these numbers. It follows therefore, that we may take such a scale for measuring the densities that the logarithms of the numbers of this scale shall be the very portions of the axis; that is, of the vertical line in feet, yards, fathoms, or what measure we please: and we may, on the other hand, choose such a scale for measuring our elevations, that the logarithms of our scale of densities shall be parts of this scale of elevations; and we may find either of these scales scientifically. For it is a known property of the logarithmic curves, that when the ordinates are the same, the intercepted portions of the abscissæ are proportional to their subtangents. Now we know the subtangent of the atmospherical logarithmic: it is the height of the homogeneous atmosphere in any measure we please, suppose fathoms: we find this height by comparing the gravities of air and mercury, when both are of some determined density. Thus, in the temperature of 32° of Fahrenheit's thermometer, when the barometer stands at 30 inches, it is known (by many experiments) that mercury is 10423,068 times heavier than air; therefore the height of the balancing column of homogeneous air will be 10423,068 times 30 inches, that is, 4342,945 English fathoms. Again, it is known that the subtangent of our common logarithmic tables, where 1 is the logarithm of the number 10, is 0,4342945. Therefore the number 0,4342945 is to the difference D of the logarithms of any two barometric heights as 4342,945 fathoms are to the fathoms F contained in the portion of the axis of the atmospherical logarithmic, which is intercepted between the ordinates equal to these barometrical heights; or that 0,4342945 : D = 4342,945 : F, and 0,4342,945 : 4342,945 = D : F; but 0,4342945 is the ten-thousandth part of 4342,945, and therefore D is the ten-thousandth part of F.

And thus it happens, by mere chance, that the logarithms of the densities, measured by the inches of mercury which their elasticity supports in the barometer, are just the ten-thousandth part of the fathoms contained in the corresponding portions of the axis of the atmospherical logarithmic. Therefore, if we multiply our common logarithms by 10000, they will express the fathoms of the axis of the atmospherical logarithmic; nothing is more easily done. Our logarithms contain what is called the index or characteristic, which is an integer and a number of decimal places. Let us just remove the integer-place four figures to the right hand: thus the logarithm of 60 is 1.7781513, which is one integer and $\frac{7781513}{10000000}$. Multiply this by 10,000, and we obtain $\frac{513}{1001}$ 17781,513, or 17781 $\frac{513}{1000}$.

The practical application of all this reasoning is obvious and easy: observe the heights of the mercury in the barometer at the upper and lower stations in inches and decimals; take the logarithms of these, and subtract the one from the other: the difference between them (accounting the four first decimal figures as integers) is the difference of elevation of fathoms.

Example.

Merc. Height at the lower station 29,8 - 1.4742163
upper station 29,1 - 1.4638930

Diff. of Log. × 10000 - - - 0.0103,233

or 103 fathoms and $\frac{233}{1000}$ of a fathom, which is 619,392 feet, or 619 feet 4¾ inches; differing from the approximated value formerly found about ½ an inch.

Such is the general nature of the barometric measurement of heights first suggested by Dr Halley; and it has been verified by numberless comparisons of the heights calculated in this way with the same height measured geometrically. It was indeed in this way that the precise specific gravity of air and mercury was most accurately determined, namely, by observing, that when the temperature of air and mercury was 32, the difference of the logarithms of the mercurial heights were precisely the fathoms of elevation. But it requires many corrections to adjust this method to the circumstances of the case; and it was not till very lately that it has been so far adjusted to them as to become useful. We are chiefly indebted to M. de Luc for the improvements. The great elevations in Switzerland enabled him to make an immense number of observations, in almost every variety of circumstances. Sir George Shuckburgh also made a great number with most accurate instruments in much greater elevations, in the same country; and he made many chamber experiments for determining the laws of variation in the subordinate circumstances. General Roy also made many to the same purpose. And to these two gentlemen we are chiefly obliged for the corrections which are now generally adopted.

It is easy to perceive, that the method, as already expressed, cannot apply to every case: it depends on the specific gravity of air and mercury, combined with the supposition that this is affected *only by a change of pressure*. But since all bodies are expanded by heat, and as there is no reason to suppose that they are equally expanded by it, it follows that a change of temperature will change the relative gravity of mercury and air, even although both suffer the same change of temperature: and since the air may be warmed or cooled when the mercury is not, or may change its temperature independent of it, we may expect still greater variations of specific gravity.

The general effect of an augmentation of the specific gravity of the mercury must be to increase the subtangent of the atmospherical logarithmic; in which case the logarithms of the densities, as measured by inches of mercury, will express measures that are greater than fathoms in the same proportion that the subtangent is increased; or, when the air is more expanded than the mercury, it will require a greater height of homogeneous atmosphere to balance 30 inches of mercury, and a given fall of mercury will then correspond to a thicker stratum of air.

In order, therefore, to perfect this method, we must learn by experiment how much mercury expands by an increase of temperature; we must also learn how much the air expands by the same, or any change of temperature; and how much its elasticity is affected by it. Both these circumstances must be considered in the case of air; for it might happen that the elasticity of the air is not so much affected by heat as its bulk is.

It will, therefore, be proper to state in this place the experiments which have been made for ascertaining these two expansions.

The most accurate, and the best adapted experiments for ascertaining the expansion of mercury, are those of General Roy, published in the 67th volume of the Philosophical Transactions. He exposed 30 inches of mercury, actually supported by the atmosphere in a barometer, in a nice apparatus, by which it could be made of one uniform temperature through its whole length; and he noted the expansion of it in decimals of an inch. These are contained in the following table; where the first column expresses the temperature by Fahrenheit's thermometer, the second column expresses the bulk of the mercury, and the third column the expansion of an inch of mercury for an increase of one degree in the adjoining temperatures.

TABLE A.

Temp.	Bulk of Mercury.	Expan. for 1°	Temp.	Bulk of Mercury.	Expan. for 1°
212°	30,5117	0,0000763	102°	30,2223	0,0001003
202	30,4888	0,0000787	92	30,1922	0,0001023
192	30,4652	0,0000810	82	30,1615	0,0001043
182	30,4409	0,0000833	72	30,1302	0,0001063
172	30,4159	0,0000857	62	30,0984	0,0001077
162	30,3902	0,0000880	52	30,0661	0,0001093
152	30,3638	0,0000903	42	30,0333	0,0001110
142	30,3367	0,0000923	32	30,0000	0,0001127
132	30,3090	0,0000943	22	29,9662	0,0001143
122	30,2807	0,0000963	12	29,9319	0,0001160
112	30,2518	0,0000983	2	29,8971	0,0001177
			0	29,8901	

This table gives rise to some reflections. The scale of the thermometer is constructed on the supposition that the successive degrees of heat are measured by equal increments of bulk in the mercury of the thermometer. How comes it, therefore, that this is not accompanied by equal increments of bulk in the mercury of the column, but that the corresponding expansions of this column do continually diminish? General Roy attributes this to the gradual detachment of elastic matter from the mercury by heat, which presses on the top of the column, and therefore shortens it. He applied a boiling heat to the vacuum a-top, without producing any farther depression; a proof that the barometer had been carefully filled. It had indeed been boiled through its whole length. He had attempted to measure the mercurial expansion in the usual way, by filling 30 inches of the tube with boiled mercury, and exposing it to the heat with the open end uppermost. But here it is evident that the expansion of the tube, and its solid contents, must be taken into the account. The expansion of the tube was found so exceedingly irregular, and so incapable of being determined with precision for the tubes which were to be employed, that he was obliged to have recourse to the method with the real barometer. In this no regard was necessary to any circumstance but the perpendicular height. There was, besides, a propriety in examining the mercury in the very condition in which it was used for measuring the pressure of the atmosphere; because whatever complication there was in the results, it was the same in the barometer in actual use.

The most obvious manner of applying these experiments on the expansion of mercury to our purpose, is to reduce the observed height of the mercury to what it would have been if it were of the temperature 32. Thus, suppose that the observed mercurial height is 29,2, and that the temperature of the mercury is 72°, make 30,1302 : 30 = 29,2 : 29,

0738. This will be the true measure of the density of the air of the standard temperature. In order that we may obtain the exact temperature of the mercury, it is proper that the observation be made by means of a thermometer attached to the barometer-frame, so as to warm and cool along with it.

Or this may be done without the help of a table, and with sufficient accuracy, from the circumstance that the expansion of an inch of mercury for one degree diminishes very nearly $\frac{1}{1000}$ part in each succeeding degree. If, therefore, we take from the expansion at 32° its thousandth part for each degree of any range above it, we obtain a mean rate of expansion for that range. If the observed temperature of the mercury is below 32°, we must *add* this correction to obtain the mean expansion. This rule will be made more exact if we suppose the expansion at 32° to be = 0,0001127. Then multiply the observed mercurial height by this expansion, and we obtain the correction, to be subtracted or added according as the temperature of the mercury was above or below 32°. Thus to abide by the former example of 72°. This exceeds 32° by 40: therefore take 40 from 0,0001127, and we have 0,0001087 for the medium expansion for that range. Multiply this by 40, and we have the whole expansion of one inch of mercury, = 0,004348. Multiply the inches of mercurial height, viz. 29,2, by this expansion, and we have for the correction 0,12696; which being subtracted from the observed height leaves 29,07304, differing from the accurate quantity less than the thousandth part of an inch. This rule is very easily kept in the memory, and supersedes the use of a table.

This correction may be made with all necessary exactness by a rule still more simple; namely, by multiplying the observed height of the mercury by the difference of its temperature from 32°, and cutting off four ciphers before the decimals of the mercurial height. This will seldom err

$\frac{1}{100}$ of an inch. We even believe that it is the most exact method within the range of temperatures that can be expected to occur in measuring heights: for it appears, by comparing many experiments and observations, that General Roy's measure of the mercurial expansion is too great, and that the expansion of an inch of mercury between 20° and 70 of Fahrenheit's thermometer does not exceed 0,000102 *per* degree. Having thus corrected the observed mercurial heights by reducing them to what they would have been if the mercury had been of the standard temperature, the logarithms of the corrected heights are taken, and their difference, multiplied by 10000, will give the difference of elevations in English fathoms.

There is another way of applying this correction, fully more expeditious and equally accurate. The difference of the logarithms of the mercurial heights is the measure of the ratio of those heights. In like manner the difference of the logarithms of the observed and corrected heights at any station is the measure of the ratio of those heights. Therefore this last difference of the logarithms is the measure of the correction of this ratio. Now the observed height is to the corrected height nearly as 1 to 1,000102. The logarithm of this ratio, or the difference of the logarithms of 1 and 1,000102, is 0,0000444. This is the correction for each degree that the temperature of the mercury differs from 32. Therefore multiply 0,0000444 by the difference of the mercurial temperatures from 32, and the products will be the corrections of the respective logarithms.

But there is still an easier way of applying the logarithmic correction. If both the mercurial temperatures are the same, the differences of their logarithms will be the same, although each may be a good deal above or below the standard temperature, if the expansion be very nearly equable. The correction will be necessary only when the temperatures at the two stations are different, and will be proportional to this difference. Therefore, if the difference of the mercurial temperatures be multiplied by 0,0000444, the product will be the correction to be made on the difference of the logarithms of the mercurial heights.

But farther, since the differences of the logarithms of the mercurial heights are also the differences of elevation in English fathoms, it follows that the correction is also a difference of elevation in English fathoms, or that the correction for one degree of difference of mercurial temperature is $\frac{444}{1000}$ of a fathom, or 32 inches, or 2 feet 8 inches.

This correction of 2.8 for every degree of difference of temperature must be subtracted from the elevation found by the general rate, when the mercury at the upper station is colder than that at the lower. For when this is the case, the mercurial column at the upper station will appear too short, the pressure of the atmosphere too small, and therefore the elevation in the atmosphere will appear greater than it really is.

Therefore the rule for this correction will be to multiply 0,0000444 by the degrees of difference between the mercurial temperatures at the two stations, and to add or subtract the product from the elevation found by the general rule, according as the mercury at the upper station is hotter or colder than that at the lower.

If the experiments of General Roy, on the expansion of the mercury in a real barometer, be thought most deserving of attention, and the expansion be considered as variable, the logarithmic difference corresponding to this expansion for the *mean* temperature of the two barometers may be taken. These logarithmic differences are contained in table B, which is carried as far as 112°, beyond which it is not probable that any observations will be made. The number for each temperature is the difference between the logarithms of 30 inches of the temperature 32, and of 30 inches expanded by that temperature.

TABLE B.

Temp.	Logarithm diff.	Dec. of Fath.	Ft. In.
112°			
	0.0000427	,427	2.7
102			
	0.0000436	,436	2.7
92			
	0.0000444	,444	2.8
82			
	0.0000453	,453	2.9
72			
	0.0000460	,460	2.9
62			
	0.0000468	,468	2.10
52			
	0.0000475	,475	2.10
42			
	0.0000482	,482	2.11
32			
	0.0000489	,489	2.11
22			
	0.0000497	,497	3.0
12			
	0.0000504	,504	3.0
0			

TABLE C.

Merc.	Diff.	Air.	Diff.
212		212,0	
	20		17,6
192		194,4	
	20		18,2
172		176,2	
	20		18,8
152		157,4	
	20		19,4
132		138,0	
	20		20,0
112		118,0	
	20		20,8
92		97,2	
	20		21,6
72		75,6	
	20		22,6
52		53,0	
	20		21,6
32		31,4	
	20		20,0
12		11,4	

It is also necessary to attend to the temperature of the air; and the change that is produced by heat in its density is of much greater consequence than that of the mercury. The relative gravity of the two, on which the subtangent of the logarithmic curve depends, and consequently the unit of our scale of elevations is much more affected by the heat of the air than by the heat of the mercury.

This adjustment is of incomparably greater difficulty than the former, and we can hardly hope to make it perfect. We shall narrate the chief experiments which have been made on the expansion of air, and deduce from them such rules as appear to be necessary consequences of them, and then notice the circumstances which leave the matter still imperfect.

General Roy compared a mercurial and an air thermometer, each of which was graduated arithmetically, that is, the units of the scales were equal bulks of mercury, and equal bulks (perhaps different from the former) of air. He found their progress as in table C.

It has been established by many experiments that equal increments of heat produce equal increments in the bulk of mercury. The differences of temperature are therefore expressed by the second column, and may be considered as equal; and the numbers of the third column must be allowed to express the same temperatures with those of the first. They *directly* express the *bulks* of the air, and the numbers of the fourth column express the differences of these bulks. These are evidently unequal, and show that common air expands most of all when of the temperature 62 nearly.

The next point was to determine what was the *actual* increase of bulk by some known increase of heat. For this purpose he took a tube, having a narrow bore, and a ball at one end. He measured with great care the capacity of both the ball and the tube, and divided the tube into equal spaces which bore a determined proportion to the capacity of the ball. This apparatus was set in a long cylinder filled with frigorific mixtures, or with water, which could be uniformly heated up to the boiling temperature, and was accompanied by a nice thermometer. The expansion of the air was measured by means of a column of mercury, which rose or sunk in the tube. The tube being of a small bore, the mercury did not drop out of it; and the bore being chosen as equable as possible, this column remained of an uniform length, whatever part of the tube it chanced to occupy. By this contrivance he was able to examine the expansibility of air of various densities. When the column of mercury contained only a single drop or two, the air was nearly of the density of the external air. If he wished to examine the expansion of air twice or thrice as dense, he used a column of 30 or 60 inches long: and to examine the expansion of air that is rarer than the external air, he placed the tube with the ball uppermost, the open end coming through a hole in the bottom of the vessel containing the mixtures or water. By this position the column of mercury was hanging in the tube, supported by the pressure of the atmosphere; and the elasticity of the included air was measured by the difference between the suspended column and the common barometer.

The following table contains the expansion of 1000 parts of air, nearly of the common density, by heating it from 0 to 212. The first column contains the height of the barometer; the second contains this height augmented by the small column of mercury in the tube of the manometer, and therefore expresses the density of the air examined; the third contains the total expansion of 1000 parts; and the fourth contains the expansion for 1°, supposing it uniform throughout.

TABLE D.

Barometer.	Density of Air examined.	Expansion of 1000 parts by 212°.	Expansion by 1°.
29,95	31,52	483,89	2,2825
30,07	30,77	482,10	2,2741
29,48	29,90	480,74	2,2676
29,90	30,73	485,86	2,2918
29,96	30,92	489,45	2,3087
29,90	30,55	476,04	2,2455
29,95	30,60	487,55	2,2998
30,07	30,60	482,80	2,2774
29,48	30,00	489,47	2,3087
Mean	30,62	484,21	2,2840

Hence it appears, that the mean expansion of 1000 parts of air, of the density 30,62 by one degree of Fahrenheit's thermometer, is 2,384, or that 1000 becomes 1002,284.

If this expansion be supposed to follow the same rate that was observed in the comparison of the mercurial and air thermometer, we shall find that the expansion of a thousand parts of air for one degree of heat at the different intermediate temperatures will be as in table E.

TABLE E.

Temper.	Total Expansion.	Expansion for 1°.
212	484,210	2,0099
192	444,011	2,0080
172	402,452	2,1475
152	359,503	2,2155
132	315,193	2,2840
112	269,513	2,3754
92	222,006	2,4211
82	197,795	2,5124
72	172,671	2,5581
62	147,090	2,6037
52	121,053	2,5124
42	95,929	2,4211
32	71,718	2,3297
22	48,421	2,2383
12	26,038	2,1696
0		

TABLE F.

Temp.	Bulk.	Differ.	Expansion for 1°.
212	13489	375	18,7
192	13474	387	19,3
172	13087	392	19,6
152	12685	413	20,6
132	12272	426	21,3
112	11846	443	22,1
92	11403	226	22,6
82	11177	235	23,5
72	10942	238	23,8
62	10704	243	24,3
52	10461	235	23,5
42	10226	226	22,6
32	10000	217	21,7
22	9783	209	20,9
12	9574	243	20,2
0	9331		

If we would have a mean expansion for any particular range, as between 12° and 92°, which is the most likely to comprehend all the geodætical observations, we need only take the difference of the bulks 26,038 and 222,006 = 195,968, and divide this by the interval of temperature 80°, and we obtain 2,4496, or 2,45, for the mean expansion for 1°.

It would perhaps be better to adapt the table to a mass of 1000 parts of air of the standard temperature 32°; for in its present form it shows the expansibility of air originally of the temperature 0. This will be done with sufficient accuracy by saying (for 212°) 1071,718 : 1484, 210 = 1000, : 13849, and so of the rest. Thus we shall construct the expansion of 10,000 parts of air as in Table F.

This will give for the mean expansion of 1000 parts of air between 12° and 92 = 2,29.

Although it cannot happen that in measuring the differences of elevation near the earth's surface, we shall have occasion to employ air greatly exceeding the common density, we may insert the experiments made by General Roy on such airs. They are expressed in Table G, where column first contains the densities measured by the inches of mercury that they will support when of the temperature 32°; column second is the expansion of 1000 parts of such air by being heated from 0 to 212; and column third is the mean expansion for 1°.

TABLE G.

	Density.	Expansion for 212.	Expansion for 1°.
	101,7	451,54	2,130
	92,3	423,23	1,996
	80, 5	412,09	1,944
	54,5	439,87	2,075
	49,7	443,24	2,091
Mean	75,7	434	2,047

TABLE H.

Temp.	Bulk.	Difference.	Expans. for 1°.
212	1141,504	7,075	0,354
192	1134,429	12,264	0,613
172	1122,165	14,150	0,708
152	1108,015	14,151	0,708
132	1093,864	14,228	0,711
112	1079,636	14,937	0,747
92	1064,699	20,911	1,045
72	1043,788	25,943	1,297
52	1017,845	17,845	0,892
32	1000,000		
	Mean expansion,		0,786

We have much more frequent occasion to operate in air that is rarer than the ordinary state of the superficial atmosphere. General Roy accordingly made many experiments on such airs. He found in general, that their expansibility by heat was analogous to that of air in its ordinary density, being greatest about the temperature 60°. He found, too, that its expansibility by heat diminished with its density, but he could not determine the law of gradation. When reduced to about one-fifth of the density of common air, its expansion was as follows:

From this very extensive and judicious range of experiments, it is evident that the expansibility of air by heat is greatest when the air is about its ordinary density, and that in small densities it is greatly diminished. It appears also, that the law of compression is altered; for in this specimen of the rare air half of the whole expansion happens about the temperature 99°, but in air of ordinary density at 105°. This being the case, we see that the experiments of Mr Amontons, narrated in the Memoirs of the Academy at Paris 1702, &c. are not inconsistent with those more perspicuous experiments of General Roy. Amontons found, that whatever was the density of the air, at least in cases much denser than ordinary air, the change of 180° of temperature increased its elasticity in the same proportion: for he found, that the column of mercury which it supported, when of the temperature 50, was increased one-third at the temperature 212. Hence he hastily concluded, that its expansibility was increased in the same proportion; but this by no means follows, unless we are certain that in every temperature the elasticity is proportional to the density. This is a point which still remains undecided; and it merits attention, because, if true, it establishes a remarkable law concerning the action of heat, which would seem to go to prove that the elasticity of fluids is the property of the matter of fire, which it superinduces on every body with which it combines in the form of vapour.

After this account of the expansion of air, we see that the height through which we must rise in order to produce a given fall of the mercury in the barometer, or the thickness of the stratum of air equiponderant with a tenth of an inch of mercury, must increase with the expansion of air; and that if $\frac{2,29}{1000}$ be the expansion for one degree, we must multiply the excess of the temperature of the air above 32° by 0,00229, and multiply the product by 87, in order to obtain the thickness of the stratum where the barometer stands at 30 inches; or whatever be the elevation indicated by the difference of the barometrical heights, upon the supposition that the air is of the temperature 32°, we must multiply this by 0,00229 for every degree that the air is warmer or colder than 32. The product must be added to the elevation in the first case, and subtracted in the latter.

Sir George Shuckburgh deduces 0,0024 from his experiments as the mean expansion of air in the ordinary cases: and this is probably nearer the truth; because General Roy's experiments were made on air which was freer from damp than the ordinary air in the fields; and it appears from his experiments, that a very minute quantity of damp increases its expansibility by heat in a prodigious degree.

The great difficulty is how to apply this correction; or rather, how to determine the temperature of the air in those extensive and deep strata in which the elevations are measured. It seldom or never happens that the stratum is of the same temperature throughout. It is commonly much colder aloft; it is also of different constitutions. Below it is warm, loaded with vapour, and very expansible; above it is cold, much drier, and less expansible, both by its dryness and its rarity. The currents of wind are often disposed in strata, which long retain their places; and as they come from different regions, are of different temperatures and different constitutions. We cannot therefore determine the expansion of the whole stratum with precision, and must content ourselves with an approximation: and the best approximation that we can make is, by supposing the whole stratum of a mean temperature between those of its upper and lower extremity, and employ the expansion corresponding to that mean temperature.

This, however, is founded on a gratuitous supposition, that the whole intermediate stratum expands alike, and that the expansion is equable in the different intermediate temperatures; but neither of these are warranted by experiment. Rare air expands less than what is denser; and therefore the general expansion of the whole stratum renders its density more uniform. Dr Horsley has pointed out some curious consequences of this in *Phil. Trans.* vol. lxiv. There is a particular elevation at which the general expansion, instead of diminishing the density of the air, increases it by the superior expansion of what is below;

and we know that the expansion is not equable in the intermediate temperatures; but we cannot find out a rule which will give us a more accurate correction than by taking the expansion for the mean temperature.

When we have done this, we have carried the method of measuring heights by the barometer as far as it can go; and this source of remaining error makes it needless to attend to some other very minute equations which theory points out. Such is the diminution of the weight of the mercury by the change of distance from the centre of the earth. This accompanies the diminution of the weight of the air, but neither so as to compensate it, nor to go along with it *pari passu*.

After all, there are found cases where there is a regular deviation from those rules, of which we cannot give any very satisfactory account. Thus it is found, that in the province of Quito in Peru, which is at a great elevation above the surface of the ocean, the heights obtained by these rules fall considerably short of the real heights; and at Spitzbergen they considerably exceed them. It appears that the air in the circumpolar regions is denser than the air of the temperate climates when of the same heat and under the same pressure; and the contrary seems to be the case with the air in the torrid zone. It would seem that the specific gravity of air to mercury is at Spitzbergen about 1 to 10224, and in Peru about 1 to 13100. This difference is with great probability ascribed to the greater dryness of the circumpolar air.

This source of error will always remain; and it is combined with another, which should be attended to by all who practise this method of measuring heights, namely a difference in the specific gravity of the quicksilver. It is thought sufficiently pure for a barometer when it is cleared of all calcinable matter, so as not to drag or sully the tube. In this state it may contain a considerable portion of other metals, particularly of silver, bismuth, and tin, which will diminish its specific gravity. It has been obtained by revivication from cinnabar of the specific gravity 14,289, and it is thought very fine if 13,65. Sir George Shuckburgh found the quicksilver, which agreed precisely with the atmospherical observations on which the rules are founded, to have the specific gravity 13,61. It is seldom obtained so heavy. It is evident that these variations will change the whole results; and that it is absolutely necessary, in order to obtain precision, that we know the density of the mercury employed. The subtangent of the atmospherical logarithmic, or the height of the homogeneous atmosphere, will increase in the same proportion with the density of the mercury; and the elevation corresponding to $\frac{1}{10}$ of an inch of barometric height will change in the same proportion.

We must be contented with the remaining imperfections: and we can readily see, that, for any purpose that can be answered by such measurements of great heights, the method is sufficiently exact; but it is quite inadequate to the purpose of taking accurate levels, for directing the construction of canals, aqueducts, and other works of this kind, where extreme precision is absolutely necessary.

We shall now deduce from all that has been said on this subject sets of easy rules for the practice of this mode of measurement, illustrating them by an example.

1. *M. de Luc's Method.*

I. Subtract the logarithm of the barometrical height at the upper station from the logarithm of that at the lower, and count the index and four first decimal figures of the remainder as fathoms, the rest as a decimal fraction. Call this the *elevation*.

II. Note the different temperatures of the mercury at the two stations, and the mean temperature. Multiply the logarithmic expansion corresponding to this mean temperature (in Table B, p. 664.) by the difference of the two temperatures, and subtract the product from the elevation if the barometer has been coldest at the upper station, otherwise add it. Call the difference or the sum the *approximated elevation.*

III. Note the difference of the temperatures of the air at the two stations by a detached thermometer, and also the mean temperature and its difference from 32°. Multiply this difference by the expansion of air for the mean temperature, and multiply the approximate elevation by 1 ± this product, according as the air is above or below 32°. The product is the correct elevation in fathoms and decimals.

Example.

Suppose that the mercury in the barometer at the lower station was at 29,4 inches, that its temperature was 50°, and the temperature of the air was 45; and let the height of the mercury at the upper station be 25,19 inches, its temperature 46, and the temperature of the air 39. Thus we have

Mercurial Hts.	Temp. Merc.		Mean.	Temp. Air.	Mean.
29,4	50	4	48	45	42
25,19	46			39	

I. Log. of 29,4		1.4683473
Log. of 25.19		1.4012282
Elevation in fathoms		671,191
II. Expans. for 48°	,473	
Multiply by	4	
		1,892
Approximated elevation		669,299
III. Expans. of air at 42°	0,00238	
× 42 — 32, = 10°	10	
	0,0238	

Multiply - - -	669,2990
By - - - -	1,0238
Product = the correct elevation -	685,228

2. *Sir George Shuckburgh's Method.*

I. Reduce the barometric heights to what they would be if they were of the temperature 32°.

II. The difference of the logarithms of the reduced barometrical heights will give the approximate elevation.

III. Correct the approximated elevation as before.

Same Example.

I. Mean expansion for 1° from Table A, p. 659, is 0,000111.

18° × 0,000111 × 29,4 = - -	0,059
Subtract this from - -	29,4
Reduced barometric height - -	29,341

Expans. from Table A, p. 659, is 0,000111.

14° × 0,000111 × 25,19 - -	0,039
Subtract from - - -	25,190
Reduced barometric height - -	25,151
II. Log. 39,341 - - -	0.4674749
Log. 25,151 - - -	1.4005553
Approximated elevation -	669,196
III. This multiplied by 1,0238, gives -	685,125

Remark 1. If 0,000101 be supposed the mean expansion of mercury for 1°, as Sir George Shuckburgh determines it, the reduction of the barometric heights will be had sufficiently exact by multiplying the observed heights of the mercury by the difference of its temperatures from 32, and cutting off four more decimal places; thus $29,4 \frac{\times 18}{10000}$ gives for the reduced height 29,347, and $25,19 \times \frac{14}{10000}$ gives 25,155, and the difference of their logarithms gives 669,4 fathoms for the approximated elevation, which differs from the one given above by no more than 15 inches.

Remark 2. If 0,0024 be taken for the expansion of air for one degree, the correction for this expansion will be had by multiplying the approximated elevation by 12, and this product by the sum of the differences of the temperatures from 32°, counting that difference as negative when the temperature is below 32, and cutting off four places; thus $669,196 \times 12 \times \overline{13+07} \times \frac{1}{10000} = 16,061$, which added to 669,196 gives 685,257, differing from the former only 9 inches.

From the same premises we may derive a rule, which is abundantly exact for all geodaetical purposes, and which requires no tables of any kind, and is easily remembered.

1. The height through which we must rise in order to produce any fall of the mercury in the barometer, is inversely proportional to the density of the air, that is, to the height of the mercury in the barometer.

2. When the barometer stands at 30 inches, and the air and quicksilver are of the temperature 32, we must rise through 87 feet, in order to produce a depression of $\frac{1}{10}$ of an inch.

3. But if the air be of a different temperature, this 87 feet must be increased or diminished by 0,21 of a foot for every degree of difference of the temperature from 32°.

4. Every degree of difference of the temperatures of the mercury at the two stations makes a change of 2,833 feet, or 2 feet 10 inches in the elevation.

Hence the following rule:

1. Take the difference of the barometric heights in tenths of an inch. Call this d.

2. Multiply the difference a between 32, and the mean temperature of the air by 21, and take the sum or difference of this product and 87 feet. This is the height through which we must rise to cause the barometer to fall from 30 inches to 29,9. Call this height h.

Let m be the mean between the two barometric heights. Then $\frac{30\,d\,h}{m}$ is the approximated elevation very nearly.

Multiply the difference δ of the mercurial temperatures by 2,83 feet, and add this product to the approximated elevation if the upper barometer has been the warmest, otherwise subtract it. The result, that is, the sum or difference, will be the corrected elevation.

Same Example.

$$d = 294 - 251{,}9 = 42{,}1$$
$$h = 87 + 10 \times 0{,}21, = 89{,}1$$
$$m = \frac{29{,}4 + 25.19}{2} = 27{,}29$$

Approx. elevation $= \frac{30 \times 42{,}1 \times 89{,}1}{27{,}29}$, $=$ 4123,24 feet.

Corr. for temp. of mercury, $= 4 \times 2{,}83$ 11,32

Corrected elevation in feet - - 4111,92

Ditto in fathoms - - - 685,32

Differing from the former only 15 inches.

This rule may be expressed by the following simple and easily-remembered formula, where a is the difference between 32^0 and the mean temperature of the air, d is the difference of barometric heights in tenths of an inch, m is the mean barometric height, δ the difference between the mercurial temperatures, and E is the correct elevation. $E = \frac{30\,(87 \pm 0,21\,a)\,d}{m} \pm \zeta \times 2,83$.

We shall now conclude this subject by an account of some of the most remarkable mountains, &c. on the earth, above the surface of the ocean, in feet.

		Feet.
Mount Puy de Domme in Auvergne, the first mountain measured by the barometer		5197
Mount Blanc	Alps	15646
Monte Rosa		15084
Aiguille d'Argenture		13402
Monastery of St Bernard		7944
Mount Cenis		9212
Pic de los Reyes	Pyrenees	7620
Pic du Medi		9300
Pic d'Ossano		11700
Canegou		8544
Lake of Geneva		1232
Mount Ætna		10963
Mount Vesuvius		3938
Mount Hecla in Iceland		4300
Snowdown		3571

	Feet.
Ben More	3870
Ben Lawers	4015
Ben Gloe	3472
Shihallion	3281
Ben Lomond	3240
Ben-nevis	4380
Cairn-gorm	4080
Tinto	1720
Table Hill, Cape of Good Hope	3454
Gondar City in Abyssinia	8440
Source of the Nile	8082
Pic of Teneriffe	14026
Chimboraçon	21440
Cayambourow	19391
Antisana	19150
Pichinha	15670
City of Quito	9977
Caspian Sea below the Ocean	306

This last is so singular, that it is necessary to give the authority on which this determination is founded. It is deduced from nine years' observations with the barometer at Astrakan by Mr Lecre, compared with a series of observations made with the same barometer at St Petersburgh.

This employment of the barometer has caused it to become a very interesting instrument to the philosopher and to the traveller; and many attempts have been made of late to improve it, and render it more portable. The improvements have either been directed to the enlargement of its range, or to the more accurate measurement of its present scale. Of the first kind are Hooke's wheel barometer, the diagonal barometer, and the horizontal barometer, described in a former volume of this work. In that place are also described two very ingenious contrivances of Mr Rownings, which are evidently not portable. Of all the barometers with an enlarged scale, the best is that invented by Dr Hooke in 1668, and described in the Phil. Trans. No 185. The invention was also claimed by Huyghens and by De la Hire; but Hooke's was published long before.

It consists of a compound tube ABCDEFG (Fig. 73.), of which the parts AB and DE are equally wide, and EFG as much narrower as we would amplify the scale. The parts AB and EG must also be as perfectly cylindrical as possible. The part HBCDI is filled with mercury, having a vacuum above in AB. IF is filled with a light fluid, and FG with another light fluid which will not mix with that in IF. The cistern G is of the same diameter as AB. It is easy to see that the range of the separating surface at F must be as much greater than that of the surface I as the area of I is greater than that of F. And this ratio is in our choice. This barometer is free from all the bad qualities of those formerly described, being most delicately moveable; and is by far the fittest for a chamber, for amusement, by observations on the changes of the atmospheric pressure. The slightest breeze causes it to rise and fall, and it is continually in motion.

But this, and all other contrivances of the kind, are inferior to the common barometer for measurement of heights, on account of their bulk and cumbersomeness; nay, they are inferior for all philosophical purposes in point of accuracy; and this for a reason that admits of no reply. Their scale must be determined in all its parts by the common barometer; and therefore, notwithstanding their great range, they are susceptible of no greater accuracy than that with which the scale of a common barometer can be observed and measured. This will be evident to any person who will take the trouble of considering how the points of their scale must be ascertained. The most accurate method for graduating such a barometer as we have now described would be to make a mixture of vitriolic acid and water, which should have $\frac{1}{10}$ of the density of mercury. Then, let a long tube stand vertical in this fluid, and connect its upper end with the open end of the barometer by a pipe which has a branch to which we can apply the mouth. Then if we suck through this pipe, the fluid will rise both in the barometer and in the other tube; and 10 inches rise in this tube will correspond to one inch descent in the common barometer. In this manner may every point of the scale be adjusted in due proportion to the rest. But it still remains to determine what particular point of the scale corresponds to some determined inch of the common barometer. This can only be done by an actual comparison; and this being done, the whole becomes equally accurate. Except therefore for the mere purpose of chamber amusement, in which case the barometer last described has a decided preference, the common barometer is to be preferred; and our attention should be entirely directed to its improvement and portability.

For this purpose it should be furnished with two microscopes, or magnifying glasses, one of them stationed at the beginning of the scale; which should either be moveable, so that it may always be brought to the surface of the mercury in the cistern, or the cistern should be so contrived that its surface may always be brought to the beginning of the scale. The glass will enable us to see the coincidence with accuracy. The other microscope must be moveable, so as to be set opposite to the surface of the mercury in the tube; and the scale should be furnished with a vernier which divides an inch into 1000 parts, and be made of materials of which we know the expansion with great precision.

For an account of many ingenious contrivances to make the instrument accurate, portable, and commodious, consult

Magellan, *Dissert. de diverses Instr. de Phys.*; *Phil. Trans.* lxvii. lxviii.; *Journ. de Phys.* xix. 108. 346. xvi. 392. xviii. 391. xxi. 436. xxii. 390.; Sulzer, Act. *Helvet.* iii. 259.; De Luc, *Recherches sur les Modifications de l'Atmosphere*, i. 401. ii. 459, 490. De Luc's seems the most simple and perfect of them all. Cardinal de Luynes (*Mem. Par.* 1768); De Luc, *Recherches*, § 63.; Van Swinden's *Positiones Physicæ*; *Com. Acad. Petrop.* i.; *Com. Acad. Petrop. Nov.* ii. 200. viii.

Thus we have given an elementary account of the distinguishing properties of air as a heavy and compressible fluid, and of the general phenomena which are immediate consequences of these properties. This we have done in a set of propositions analogous to those which form the doctrines of hydrostatics. It remains to consider it in another point of view, namely, as moveable and inert. The phenomena consequent on these properties are exhibited in the velocities which air acquires by pressure, in the resistance which bodies meet with to their motion through the air, and in the impression which air in motion gives to bodies exposed to its action.

We shall first consider the motions of which air is susceptible when the equilibrium of pressure (whether arising from its weight or its elasticity) is removed; and, in the next place, we shall consider its action on solid bodies exposed to its current, and the resistance which it makes to their motion through it.

In this consideration we shall avoid the extreme of generality, which renders the discussion too abstract and difficult, and adapt our investigation to the circumstances in which compressible fluids (of which air is taken for the representative) are most commonly found. We shall consider air therefore as it is commonly found in accessible situations, as acted on by equal and parallel gravity; and we shall consider it in the same order in which water is treated in a system of hydraulics.

In that science the leading problem is to determine with what velocity the water will move through a given orifice when impelled by some known pressure; and it has been found, that the best form in which this most difficult and intricate proposition can be put, is to determine the velocity of water flowing through this orifice when impelled by its weight alone. Having determined this, we can reduce to this case every question which can be proposed; for, in place of the pressure of any piston or other mover, we can always substitute a perpendicular column of water or air whose weight shall be equal to the given pressure.

The first problem, therefore, is to determine with what velocity air will rush into a void when impelled by its weight alone. This is evidently analogous to the hydraulic problem of water flowing out of a vessel.

And here we must be contented with referring our readers to the solutions which have been given of that problem, and the demonstration that it flows with the velocity which a heavy body would acquire by falling from a height equal to the depth of the hole under the surface of the water in the vessel. In whatever way we attempt to demonstrate that proposition, every step, nay, every word, of the demonstration applies equally to the air, or to any fluid whatever. Or, if our readers should wish to see the connexion or analogy of the cases, we only desire them to recollect an undoubted maxim in the science of motion, that *when the moving force and the matter to be moved vary in the same proportion, the velocity will be the same.* If therefore there be similar vessels of air, water, oil, or any other fluid, all of the height of a homogeneous atmosphere, they will all run through equal and similar holes with the same velocity; for in whatever proportion the quantity of matter moving through the hole be varied by a variation of density, the pressure which forces it out, by acting in circumstances perfectly similar, varies in the same proportion by the same variation of density.

We must therefore assume it as the leading proposition, that *air rushes from the atmosphere into a void with the velocity which a heavy body would acquire by falling from the top of a homogeneous atmosphere.*

It is known that air is about 840 times lighter than water, and that the pressure of the atmosphere supports water at the height of 33 feet nearly. The height therefore of a homogeneous atmosphere is nearly 33 × 840, or 27720 feet. Moreover, to know the velocity acquired by any fall, recollect that a heavy body by falling one foot acquires the velocity of 8 feet per second; and that the velocities acquired by falling through different heights are as the square roots of the heights. Therefore, to find the velocity corresponding to any height, expressed in feet per second, multiply the square root of the height by 8. We have therefore in the present instance $V = 8\sqrt{27720}, = 8 \times 166,493, = 1332$ feet per second. This therefore is the velocity with which common air will rush into a void; and this may be taken as a standard number in pneumatics, as 16 and 32 are standard numbers in the general science of mechanics, expressing the action of gravity at the surface of the earth.

It is easy to see that greater precision is not necessary in this matter. The height of a homogeneous atmosphere is a variable thing, depending on the temperature of the air. If this reason seems any objection against the use of the number 1332, we may retain $8\sqrt{H}$ in place of it, where H expresses the height of a homogeneous atmosphere of the given temperature. A variation of the barometer makes no change in the velocity, nor in the height of the homogeneous atmosphere, because it is accompanied by a proportional variation in the density of the air. When it is increased $\frac{1}{10}$, for instance, the density is also increased $\frac{1}{10}$; and thus the expelling force and the matter to be moved are changed in the same proportion, and the velocity remains the same. N. B. We do not here consider the velocity which the air acquires after its issuing into the void by its continual expansion. This may be ascertained by the 39th proposition of Newton's *Principia*, b. i. Nay, which appears very paradoxical, if a cylinder of air, communicating in this manner with a void, be compressed by a piston loaded with a weight, which presses it down as the air flows out, and thus keeps it of the same density, the velocity of efflux will still be the same, however great the pressure may chance to be: for the first and immediate effect of the load on the piston is to reduce the air in the cylinder to such a density that its elasticity shall exactly balance the load; and because the elasticity of air is proportional to its density, the density of the air will be increased in the same proportion with the load, that is, with the expelling power (for we are neglecting at present the weight of the included air as too inconsiderable to have any sensible effect.) Therefore, since the matter to be moved is increased in the same proportion with the pressure, the velocity will be the same as before.

It is equally easy to determine the velocity with which the air of the atmosphere will rush into a space containing rarer air. Whatever may be the density of this air, its elasticity, which follows the proportion of its density, will balance a proportional part of the pressure of the atmosphere; and it is the excess of this last only which is the moving force. The matter to be moved is the same as before. Let D be the natural density of the air, and δ the density of the air contained in the vessel into which it is supposed to run, and let P be the pressure of the atmosphere, and therefore equal to the force which impels it into a void; and let ϖ be the force with which this rarer air would run into a void. We have $D:\delta = P:\varpi$, and $\varpi = \frac{P\delta}{D}$. Now the moving force in the present instance is $P - \varpi$, or $P - \frac{P\delta}{D}$. Lastly, let V be the velocity of air rushing into a void, and v the velocity with which it will rush into this rarefied air.

It is a theorem in the motion of fluids, that the pressures are as the squares of the velocities of efflux. Therefore $P : P - \frac{P\delta}{D} = V^2 : v^2$. Hence we derive $v^2 = V^2 \times \overline{1 - \frac{\delta}{D}}$, and $v = V \times \sqrt{1 - \frac{\delta}{D}}$. We do not here consider the resistance which the air of the atmosphere will meet with from the inertia of that in the vessel which it must displace in its motion.

Here we see that there will always be a current into the vessel while δ is less than D.

We also learn the gradual diminution of the velocity as the vessel fills; for δ continually increases, and therefore $1 - \frac{\delta}{D}$ continually diminishes.

It remains to determine the time t expressed in seconds, in which the air of the atmosphere will flow into this vessel from its state of vacuity till the air in the vessel has acquired any proposed density δ.

For this purpose let H, expressed in feet, be the height through which a heavy body must fall in order to acquire the velocity V. expressed also in feet per second. This we shall express more briefly in future, by calling it the height producing the velocity V. Let C represent the capacity of the vessel, expressed in cubic feet, and O the area or section of the orifice, expressed in superficial or square feet; and let the natural density of the air be D.

Since the quantity of aerial matter contained in a vessel depends on the capacity of the vessel and the density of the air jointly, we may express the air which would fill this vessel by the symbol CD when the air is in its ordinary state, and by $C\delta$ when it has the density δ. In order to obtain the rate at which it fills, we must take the fluxion of this quantity $C\dot{\delta}$. This is $C\dot{\delta}$; for C is a constant quantity, and δ is a variable or flowing quantity.

But we also obtain the rate of influx by our knowledge of the velocity, and the area of the orifice, and the density. The velocity is V, or $8\sqrt{H}$, at the first instant, and when the air in the vessel has acquired the density δ, that is, at the end of the time t, the velocity is $8\sqrt{H}\sqrt{1-\frac{\delta}{D}}$, or $8\sqrt{H}\sqrt{\frac{D-\delta}{D}}$, or $8\sqrt{H}\frac{\sqrt{D-\delta}}{\sqrt{D}}$,

The rate of influx therefore (which may be conceived as measured by the little mass of air which will enter during the time $\dot{t}$ with this velocity) will be $\frac{8\sqrt{H}OD\sqrt{D-\delta}\,\dot{t}}{\sqrt{D}}$, or $8\sqrt{H}O\sqrt{D}\sqrt{D-\delta}\,\dot{t}$, multiplying the velocity by the orifice and by the density.

Here then we have two values of the rate of influx. By stating them as equal we have a fluxionary equation, from which we may obtain the fluents, that is, the time t in seconds necessary for bringing the air in the vessel to the density δ, or the density δ which will be produced at the end of any time t. We have the equation $8\sqrt{HO}\sqrt{D}\sqrt{D-\delta}\,\dot{t} = C\dot{\delta}$. Hence we derive $\dot{t} = \frac{C}{8\sqrt{HO}\sqrt{D}} \times \frac{\dot{\delta}}{\sqrt{D-\delta}}$. Of this the fluent is $t = \frac{C}{4\sqrt{HO}\sqrt{D}} \times \overline{\sqrt{D-\delta}} + A$, in which A is a conditional constant quantity. The condition which determines it is, that t must be nothing when δ is nothing, that is, when $\sqrt{D-\delta} = \sqrt{D}$; for this is evidently the case at the beginning of the motion. Hence it follows, that the constant quantity is $\sqrt{D}$, and the complete fluent, suited to the case, is

$$\frac{C}{4\sqrt{HO}\sqrt{D}} \times \overline{\sqrt{D}-\sqrt{D-\delta}}.$$

The motion ceases when the air in the vessel has acquired the density of the external air; that is, when $t =$ D, or when $t = \frac{C}{4\sqrt{HO}\sqrt{D}} \times \sqrt{D}, = \frac{C}{4\sqrt{HO}}$. Therefore the time of completely filling the vessel is $\frac{C}{4\sqrt{HO}}$.

Let us illustrate this by an example in numbers.

Supposing then that air is 840 times lighter than water, and the height of the homogeneous atmosphere 27720 feet, we have $4\sqrt{H} = 666$. Let us further suppose the vessel to contain 8 cubic feet, which is nearly a wine hogshead, and that the hole by which the air of the ordinary density, which we shall make $= 1$, enters is an inch square, or $\frac{1}{144}$ of a square foot. Then the time in seconds of completely filling it will be $\frac{8''}{\frac{1}{144}666}$, or $\frac{1152''}{666}$, or 1,7297″. If the hole is only $\frac{1}{100}$ of a square inch, that is, if its side is $\frac{1}{10}$ of an inch, the time of completely filling the hogshead will be 173″ very nearly, or something less than three minutes.

If we make the experiment with a hole cut in a thin plate, we shall find the time greater nearly in the proportion of 63 to 100, for reasons obvious to all who have studied hydraulics. In like manner we can tell the time necessary for bringing the air in the vessel to $\frac{3}{4}$ of its ordinary density. The only variable part of our fluent is the co-efficient $-\sqrt{D - z}$, or $\sqrt{1 - z}$. Let z be $= \frac{3}{4}$, then $\sqrt{1 - z} = \sqrt{\frac{1}{4}} = \frac{1}{2}$, and $1 - \sqrt{1 - z} \doteq \frac{1}{2}$; and the time is $86\frac{1}{2}''$ very nearly when the hole is $\frac{1}{10}$ of an inch wide.

Let us now suppose that the air in the vessel ABCD (Fig. 81.) is compressed by a weight acting on the cover AD, which is moveable down the vessel, and is thus expelled into the external air.

The immediate effect of this external pressure is to compress the air and give it another density. The density D of the external air corresponds to its pressure P. Let the additional pressure on the cover of the vessel be p, and the density of the air in the vessel be d. We shall have $P : P + p = D : d$; and therefore $p = P \times \frac{d-D}{D}$. Then, because the pressure which expels the air is the difference between the force which compresses the air in the vessel and the force which compresses the external air, the expelling force is p. And because the quantities of motion are as the forces which similarly produce them, we shall have $P : P \times \frac{d-D}{D} = MV : mv$; where M and m express the quantities of matter expelled, V expresses the velocity with which air rushes into a void, and v expresses the velocity sought. But because the quantities of aerial matter which issue from the same orifice in a moment are as the densities and velocities jointly, we shall have $MV : mv = DVV : dvv, = DV^2 : dv^2$. Therefore $P : p \frac{d-D}{D} = DV^2 : dv^2$. Hence we deduce $v = V \sqrt{\frac{d-D}{d}}$.

We may have another expression of the velocity without considering the density. We had $P : P + p = D : d$: therefore $d = \frac{D \times \overline{P+p}}{P}$, and $d - D = \frac{D \times \overline{P+p}}{P} - D$, $= \frac{D \times \overline{P+p} - DP}{P}$, and $\frac{d-D}{d} = \frac{D \times \overline{P+p} - DP}{D \times \overline{P+p}}$, $= \frac{P+p-P}{P+p}$, $= \frac{p}{P+p}$: therefore $v = V \times \sqrt{\frac{p}{P+p}}$, which is a very simple and convenient expression.

Hitherto we have considered the motion of air as produced by its weight only. Let us now consider the effect of its elasticity.

Let ABCD (Fig. 81.) be a vessel containing air of any density D. This air is in a state of compression; and if the compressing force be removed, it will expand, and its elasticity will diminish along with its density. Its elasticity in any state is measured by the force which keeps it in that state. The force which keeps common air in its ordinary density is the weight of the atmosphere, and is the same with the weight of a column of water 33 feet high. If therefore we suppose that this air, instead of being confined by the top of the vessel, is pressed down by a moveable piston carrying a column of water 33 feet high, its elasticity will balance this pressure as it balances the pressure of the atmosphere; and as it is a fluid, and propagates through every part the pressure exerted on any one part, it will press on any little portion of the vessel by its elasticity in the same manner as when loaded with this column.

The consequence of this reasoning is, that if this small portion of the vessel be removed, and thus a passage be made into a void, the air will begin to flow out with the same velocity with which it would flow when impelled by its weight alone, or with the velocity acquired by falling from the top of a homogeneous atmosphere, or 1332 feet in a second nearly.

But as soon as some air has come out, the density of the remaining air is diminished, and its elasticity is diminished; therefore the expelling force is diminished. But the matter to be moved is diminished in the very same proportion, because the density and elasticity are found to vary according to the same law; therefore the velocity will continue the same from the beginning to the end of the efflux.

This may be seen in another way. Let P be the pressure of the atmosphere, which being the counterbalance and measure of the initial elasticity, is equal to the expelling force at the first instant. Let D be the initial density, and V the initial velocity. Let d be its density at the end of the time t of efflux, and v the contemporaneous velocity. It is plain that at the end of this time we shall have the expelling force $\pi = \frac{P\,d}{D}$; for $D : d = P : \pi \left(= \frac{P\,d}{D}\right)$.

These forces are proportional to the quantities of motion which they produce; and the quantities of motion are proportional to the quantities of matter M and m and the velocities V and v jointly: therefore we have $P : \frac{P\,d}{D} = MV : m\,v$. But the quantities of matter which escape through a given orifice are as the densities and velocities jointly; that is, $M : m = DV : d\,v$: therefore $P : \frac{P\,d}{D} = DV^2 : dv^2$, and $P \times d\,v^2 = \frac{P\,d\,DV^2}{D} = P\,d\,V^2$, and $V^2 = v^2$, and $V = v$, and the velocity of efflux is constant. Hence follows, what appears very unlikely at first sight, that however much the air in the vessel is condensed, it will always issue into a void with the same velocity.

In order to find the quantity of aerial matter which will issue during any time t, and consequently the density of the remaining air at the end of this time, we must get the rate of efflux. In the element of time $\dot{t}$ there issues (by what has been said above) the bulk $8\sqrt{HO}\,\dot{t}$ (for the velocity V is constant); and therefore the quantity $8\sqrt{HO}\,d\,\dot{t}$. On the other hand, the quantity of air at the beginning was CD, C being the capacity of the vessel; and when the air has acquired the density d, the quantity is $C\,d$, and the quantity run out is $CD - C\,d$: therefore the quantity which has run out in the time $\dot{t}$ must be the fluxion of $CD - C\,d$, or $-C\,\dot{d}$. Therefore we have the equation $8\sqrt{HO}\,d\,\dot{t} = -C\,\dot{d}$, and $\dot{t} = \frac{-C\,\dot{d}}{8\sqrt{HO}\,d}$; $= \frac{C}{8\sqrt{HO}} \times -\frac{\dot{d}}{d}$

The fluent of this is $t = \frac{C}{8\sqrt{HO}}$ log. d. This fluent must be so taken that t may be $= o$ when $d = D$. Therefore the correct fluent will be $t = \frac{C}{8\sqrt{HO}}$ log. $\frac{D}{d}$, for log. $\frac{D}{D} =$ log 1, $= o$. We deduce from this, that it requires an infinite time for the whole air of a vessel to flow out of it into a void. *N. B.*—By log. d, &c. is meant the hyperbolic logarithm of d, &c.

Let us next suppose that the vessel, instead of letting out its air into a void, emits it into air of a less density, which remains constant during the efflux, as we may suppose to be the case when a vessel containing condensed air emits it into the surrounding atmosphere. Let the initial density of the air in the vessel be δ, and that of the atmospere D. Then it is plain that the expelling force is $P - \frac{PD}{\delta}$, and that after the time t it is $\frac{Pd}{\delta} - \frac{PD}{\delta}$. We have therefore $P - \frac{PD}{\delta} : \frac{Pd}{\delta} - \frac{PD}{\delta} = MV : mv, = \delta V^2 : d v^2$. Whence we derive $v = V\sqrt{\frac{\delta\,\overline{d-D}}{d\,\overline{\delta-D}}}$.

From this equation we learn that the motion will be at an end when $d = D$: and if $\delta = D$ there can be no efflux.

To find the relation between the time and the density, let H, as before, be the height producing the velocity V. The height producing the velocity of efflux v must be $H \times \frac{\delta\,\overline{d-D}}{d\,\overline{\delta-D}}$, and the little parcel of air which will flow out in the time $\dot t$ will be $= 8\sqrt{HO}\,d\,\dot t\sqrt{\frac{\delta\,\overline{d-D}}{d\,\overline{\delta-D}}}$. On the other hand, it is $= -C\dot d$.

Hence we deduce the fluxionary equation $\dot t = \frac{C\sqrt{\delta-D}}{8\sqrt{HO}\sqrt{\delta}} \times \frac{-\dot d}{\sqrt{d^2-Dd}}$. The fluent of this, corrected so as to make $t = O$ when $d = \delta$, is $t = \frac{C\sqrt{\delta - D}}{8\sqrt{HO}\sqrt{\delta}} \times$ log. $\left(\frac{\delta - \frac{1}{2}D + \sqrt{\delta^2 - D\delta}}{d - \frac{1}{2}D + \sqrt{d^2 - Dd}}\right)$. And the time of completing the efflux, when $d = D$, is $t = \frac{C\sqrt{\delta - D}}{8\sqrt{HO}\sqrt{\delta}} \times$ log. $\left(\frac{\delta - \frac{1}{2}D + \sqrt{\delta^2 - D\delta}}{\frac{1}{2}D}\right)$.

Lastly, Let ABCD, CFGH (Fig. 82.) be two vessels containing airs of different densities, and communicating by the orifice C, there will be a current from the vessel containing the denser air into that containing the rarer: suppose from ABCD into CFGH.

Let P be the elastic force of the air in ABCD, Q its density, and V its velocity, and D the density of the air in CFGH. And, after the time t, let the density of the air in ABCD be q, its velocity v, and the density of the air in CFGH be δ. The expelling force from ABCD will be $P - \frac{PD}{Q}$ at the first instant, and at the end of the time t it will be $\frac{Pq}{Q} - \frac{P\delta}{Q}$. Therefore we shall have $P - \frac{PD}{Q} : \frac{Pq}{Q} - \frac{P\delta}{Q} = QV^2 : qv^2$, which gives $v = V \times \sqrt{\frac{Q(q-\delta)}{q(Q-D)}}$, and the motion will cease when $\delta = q$.

Let A be the capacity of the first vessel, and B that of the second. We have the second equation $AQ + BD = Aq + B\delta$, and therefore $\delta = \frac{A(Q-q) + BD}{B}$. Substituting this value of δ in the former value of v, we have $v = V \times \sqrt{\frac{Q\left[B(q-D) - A(Q-q)\right]}{qB(Q-D)}}$, which gives the relation between the velocity v and the density q.

In order to ascertain the time when the air in ABCD has acquired the density q, it will be convenient to abridge the work by some substitutions. Therefore make Q (B+A) = M, BQD + BQ² = N, BQ — BD = R and $\frac{\mathrm{N}}{\mathrm{M}} \mp m$. Then, proceeding as before, we obtain the fluxionary equation $8\sqrt{\mathrm{HO}}\,q\,\frac{\sqrt{\mathrm{M}q-\mathrm{N}}}{\sqrt{\mathrm{R}}\sqrt{q}}\,\dot{t} = \dot{\overline{\mathrm{AQ}-\mathrm{A}q}}$ $= -\mathrm{A}\dot{q}$ whence $\dot{t} = \frac{\mathrm{A}\sqrt{\mathrm{R}}}{8\sqrt{\mathrm{HO}}\sqrt{\mathrm{M}}} \times \frac{\dot{q}}{\sqrt{q^2 - m\,q}}$ of which the fluent, completed so that $t \mp o$ when $q \mp \mathrm{Q}$, is $t =$ $\frac{\mathrm{A}\sqrt{\mathrm{R}}}{8\sqrt{\mathrm{HO}}\sqrt{\mathrm{M}}} \times \mathrm{Log.}\left(\frac{\mathrm{Q}-\frac{1}{2}m+\sqrt{(\mathrm{Q}^2 m\mathrm{Q})}}{q-\frac{1}{2}m+\sqrt{(q^2\,m\,q)}}\right)$.

Some of these questions are of difficult solution, and they are not of frequent use in the more important and usual applications of the doctrines of pneumatics, at least in their present form. The cases of greatest use are when the air is expelled from a vessel by an external force, as when bellows are worked, whether of the ordinary form or consisting of a cylinder fitted with a moveable piston. This last case merits a particular consideration; and, fortunately, the investigation is extremely easy.

Let AD, Fig. 81. be considered as a piston moving downward with the uniform velocity *f*, and let the area of the piston be *n* times the area of the hole of efflux, then the velocity of efflux arising from the motion of the piston will be *nf*. Add this to the velocity V produced by the elasticity of the air in the first question, and the whole velocity will be V + *nf*. It will be the same in the others. The problem is also freed from the consideration of the time of efflux. For this depends now on the velocity of the piston. It is still, however, a very intricate problem to ascertain the relation between the time and the density, even though the piston is moving uniformly; for at the beginning of the motion the air is of common density. As the piston descends, it both expels and compresses the air, and the density of the air in the vessel varies in a very intricate manner, as also its resistance or reaction on the piston.

For this reason, a piston which moves uniformly by means of an external force will never make an uniform blast by successive strokes; it will always be weaker at the beginning of the stroke. The best way for securing an uniform blast is to employ the external force only for lifting up the piston, and then to let the piston descend by its own weight. In this way, it will quickly sink down, compressing the air, till its density and corresponding elasticity exactly balance the weight of the piston. After this the piston will descend equably, and the blast will be uniform. These observations and theorems will serve to determine the initial velocity of the air in all important cases of its expulsion. The philosopher will learn the rate of its efflux out of one vessel into another; the chemist will be able to calculate the quantities of the different gases which are employed in the curious experiments of the ingenious but unfortunate Lavoisier on Combustion, and will find them extremely different from what he supposed; the engineer will learn how to proportion the motive force of this machine to the quantity of aerial matter which his bellows must supply. But it is not enough, for this purpose, that the air *begin* to issue in the proper quantity; we must see whether it be not affected by the circumstances of its subsequent passage.

All the modifications of motion which are observed in water conduits take place also in the passage of air through pipes and holes of all kinds. There is the same diminution of quantity passing through a hole in a thin plate that is observed in water. We know that (abating the small effect of friction) water issues with the velocity acquired by falling from the surface; and yet if we calculate by this velocity and by the area of the orifice, we shall find the quantity of water deficient nearly in the proportion of 63 to 100. This is owing to the water pressing towards the orifice from all sides, which occasions a contraction of the jet. The same thing happens in the efflux of air. Also the motion of water is greatly impeded by all contractions of its passage. These oblige it to accelerate its velocity, and therefore require an increase of pressure to force it through them, and this in proportion to the squares of the velocities. Thus, if a machine working a pump causes it to give a certain number of strokes in a minute, it will deliver a determined quantity of water in that time. Should it happen that the passage of the water is contracted to one half in any part of the machine (a thing which frequently happens at the valves), the water must move through this contraction with twice the velocity that it has in the rest of the passage. This will require four times the force to be exerted on the piston. Nay (which will appear very odd, and is never suspected by engineers), if no part of the passage is narrower than the barrel of the pump, but on the contrary a part much wider, and if the conduit be again contracted to the width of the barrel, an additional force must be applied to the piston to drive the water through this passage, which would not have been necessary if the passage had not been widened in any part. It will require a force equal to the weight of a column of water of the height necessary for communicating a velocity the square of which is equal to the difference of the squares of the velocities of the water in the wide and the narrow part of the conduit.

The same thing takes place in the motion of air, and therefore all contractions and dilatations must be carefully avoided, when we want to preserve the velocity unimpaired.

Air also suffers the same retardation in its motion along pipes. By not knowing, or not attending to that, engineers of the first reputation have been prodigiously disappointed in their expectations of the quantity of air which will be delivered by long pipes. Its extreme mobility and lightness hindered them from suspecting that it would suffer any sensible retardation. Dr Papin, a most ingenious man, proposed this as the most effectual method of transferring the action of a moving power to a great distance. Suppose, for instance, that it was required to raise water out of a mine by a water-machine, and that there was no fall of water nearer than a mile's distance. He employed this water to drive a piston, which should compress the air in a cylinder communicating, by a long pipe, with another cylinder at the mouth of the mine. This second cylinder had a piston in it, whose rod was to give motion to the pumps at the mine. He expected, that as soon as the piston at the water-machine had compressed the air sufficiently, it would cause the air in the cylinder at the mine to force up its piston, and thus work the pumps. Dr Hooke made many objections to the method when laid before the Royal Society, and it was much debated there. But dynamics was at this time an infant science, and very little understood. Newton had not then taken any part in the business of the society, otherwise the true objections would not have escaped his sagacious mind. Notwithstanding Papin's great reputation as an engineer and mechanic, he could not bring his scheme into use in England; but afterwards, in France and in Germany, where he settled, he got some persons of great fortunes to employ him in this project; and he erected great machines in Auvergne and Westphalia for draining mines. But, so far from being effective machines, they would not even begin to move. He attributed the failure to the quantity of air in the pipe of communication, which must be condensed before it can condense the air in the remote cylinder. This indeed is true, and he should have thought of this earlier. He therefore diminished the size of this pipe, and made his water-machine exhaust instead of condensing, and had no doubt but that the immense velocity with which air rushes into a void would make a rapid and effectual communication of power. But he was equally disappointed here, and the machine at the mine stood still as before.

Near a century after this, a very intelligent engineer attempted a much more feasible thing of this kind at an iron-foundery in Wales. He erected a machine at a powerful fall of water, which worked a set of cylinder bellows, the blow-pipe of which was conducted to the distance of a mile and a half, where it was applied to a blast furnace. But notwithstanding every care to make the conducting pipe very air-tight, of great size, and as smooth as possible, it would hardly blow out a candle. The failure was ascribed to the impossibility of making the pipe air-tight. But, what was surprising, above ten minutes elapsed after the action of the pistons in the bellows before the least wind could be perceived at the end of the pipe; whereas the engineer expected an interval of 6 seconds only.

No very distinct theory can be delivered on this subject; but we may derive considerable assistance in understanding the causes of the obstruction to the motion of water in long pipes, by considering what happens to air. The elasticity of the air, and its great compressibility, have given us the distinctest notions of fluidity in general, showing us, in a way that can hardly be controverted, that the particles of a fluid are kept at a distance from each other, and from other bodies, by the corpuscular forces. We shall therefore take this opportunity to give a view of the subject, which did not occur to us when treating of the motion of water in pipes, reserving a further discussion to the articles RIVER, WATERWORKS.

The writers on hydrodynamics have always considered the obstruction to the motion of fluids along canals of any kind, as owing to something like the friction by which the motion of solid bodies on each other is obstructed; but we cannot form to ourselves any distinct notion of resemblance, or even analogy, between them. The fact is, however, that a fluid running along a canal has its motion obstructed; and that this obstruction is greatest in the immediate vicinity of the solid canal, and gradually diminishes to the middle of the stream. It appears, therefore, that the parts of fluids can no more move among each other than among solid bodies, without suffering a diminution of their motion. The parts in physical contact with the sides and bottom are retarded by these immoveable bodies. The particles of the next stratum of fluid cannot preserve their initial velocities without overpassing the particles of the first stratum; and it appears from the fact that they are by this means retarded. They retard in the same manner the particles of the third stratum, and so on to the middle stratum or thread of fluid. It appears from the fact, therefore, that this sort of friction is not a consequence of rigidity alone, but that it is equally competent to fluids. Nay, since it is a matter of fact in air, and is even more remarkable there than in any other fluid, as we shall see by the experiments which have been made on the subject; and as our experiments on the compression of air show us the particles of air ten times nearer to each other in some cases than in others (viz. when we see air a thousand times denser in these cases), and therefore force us to acknowledge that they are not in contact; it is plain that this obstruction has no analogy to friction, which supposes roughness or inequality of surface. No such inequality can be supposed in the surface of an aerial particle; nor would it be of any service in explaining the obstruction, since the particles do not rub on each other, but pass each other at some small and imperceptible distance.

We must therefore have recourse to some other mode of explication. We shall apply this to air only in this place; and, since it is proved by the uncontrovertible experiments of Canton, Zimmerman, and others, that water, mercury, oil, &c. are also compressible and perfectly elastic, the argument from this principle, which is conclusive in air, must equally explain the similar phenomenon in hydraulics.

The most highly-polished body which we know must be conceived as having an uneven surface when we compare it with the small spaces in which the corpuscular forces are exerted; and a quantity of air moving in a polished pipe may be compared to a quantity of small shot sliding down a channel with undulated sides and bottom. The row of particles immediately contiguous to the sides will therefore have an undulated motion: but this undulation of the contiguous particles of air will not be so great as that of the surface along which they glide; for not only every motion requires force to produce it, but also every change of motion. The particles of air resist this change from a rectilineal to an undulating motion; and, being elastic, that is, repelling each other and other bodies, they keep a little nearer to the surface as they are passing over an eminence, and their path is less incurvated than the surface. The difference between the motion of the particles of air and the particles of a fluid quite unelastic is, in this respect, somewhat like the difference between the motion of a spring-carriage and that of a common carriage. When the common carriage passes along a road not perfectly smooth, the line described by the centre of gravity of the carriage keeps perfectly parallel to that described by the axis of the wheels, rising and falling along with it. Now let a spring body be put on the same wheels and pass along the same road. When the axis rises over an eminence perhaps half an inch, sinks down again into the next hollow, and then rises a second time, and so on, the centre of gravity of the body describes a much straighter line; for upon the rising of the wheels, the body resists the motion, and compresses the springs, and thus remains lower than it would have been had the springs not been interposed. In like manner, it does not sink so low as the axle does when the wheels go into a hollow. And thus the motion of spring-carriages becomes less violently undulated than the road along which they pass. This illustration will, we hope, enable the reader to conceive how the deviation of the particles next to the sides and bottom of the canal from a rectilineal motion is less than that of the canal itself.

It is evident that the same reasoning will prove that the undulation of the next row of particles will be less than that of the first, that the undulation of the third row will be less than that of the second, and so on, as is represented in Fig. 83. And thus it appears, that while the mass of air has a progressive motion along the pipe or canal, each particle is describing a waving line, of which a line parallel to the direction of the canal is the axis, cutting all these undulations. This axis of each undulated path will be straight or curved as the canal is, and the excursions of the path on each side of its axis will be less and less as the axis of the path is nearer to the axis of the canal.

Let us now see what *sensible* effect this will have; for all the motion which we here speak of is imperceptible. It is demonstrated in mechanics, that if a body moving with any velocity be deflected from its rectilineal path by a curved and perfectly smooth channel, to which the rectilineal path is a tangent, it will proceed along this channel with undiminished velocity. Now the path, in the present case, may be considered as perfectly smooth, since the particles do not touch it. It is one of the undulations which we are considering, and we may at present conceive this as without any subordinate inequalities. There should not, therefore, be any diminution of the velocity. Let us grant this of the absolute velocity of the particle; but what we observe is the velocity of the mass, and we judge of it perhaps by the motion of a feather carried along by it. Let us suppose a single atom to be a sensible object, and let us attend to two such particles, one at the side, and the other in the middle: although we cannot perceive the undulations of these particles during their progressive motions, we see the progressive motions themselves. Let us suppose then that the middle particle has moved without any undulation whatever, and that it has advanced ten feet. The lateral particle will also have moved ten feet; but this has not been in a straight line. It will not be so far advanced, therefore, in the direction of the canal; it will be left behind, and will appear to us to have been retarded in its motion: and in like manner each thread of particles will be more and more retarded (apparently only) as it recedes farther from the axis of the canal, or what is usually called the thread of the stream.

And thus the observed fact is shown to be a necessary consequence of what we know to be the nature of a compressible or elastic fluid; and that without supposing any diminution in the real velocity of each particle, there will be a diminution of the velocity of the sensible threads of the general stream, and a diminution of the whole quantity of air which passes along it during a given time.

Let us now suppose a parcel of air impelled along a pipe, which is *perfectly* smooth, out of a larger vessel, and issuing from this pipe with a certain velocity. It requires a certain force to change its velocity in the vessel to the greater velocity which it has in the pipe. This is abundantly demonstrated. How long soever we suppose this pipe, there will be no change in the velocity, or in the force, to keep it up. But let us suppose, that about the middle of this pipe there is a part of it which has suddenly got an undulated surface, however imperceptible. Let us further suppose that the final velocity of the middle thread is the same as before. In this case, it is evident that the sum-total of the motions of all the particles is greater than before, because the absolute motions of the lateral particles is greater than that of the central particle, which we suppose the same as before. This absolute increase of motion cannot be without an increase of propelling force: the force acting now, therefore, must be greater than the force acting formerly. Therefore, if only the former force had continued to act, the same motion of the central particle could not have been preserved, or the progressive motion of the whole stream must be diminished.

And thus we see that this internal insensible undulatory motion becomes a real obstruction to the sensible motion which we observe, and occasions an expense of power.

Let us see what will be the consequence of extending this obstructing surface further along the canal. It must evidently be accompanied by an augmentation of the motion produced, if the central velocity be still kept up; for the particles which are now in contact with the sides do not continue to occupy that situation: the middle particles moving faster *forward* get over them, and in their turn come next the side; and as they are really moving equally fast, but not in the direction into which they are now to be forced, force is necessary for changing the direction also; and this is in addition to the force necessary for producing the undulations so minutely treated of. The consequence of this must be, that an additional force will be necessary for preserving a given progressive motion in a longer *obstructing* pipe, and that the motion produced in a pipe of greater length by a given force will be less than in a shorter one, and the efflux will be diminished.

There is another consideration which must have an influence here. Nothing is more irrefragably demonstrated than the necessity of an additional force for producing an efflux through any contraction, even though it should be succeeded by a dilatation of the passage. Now both the inequalities of the sides and the undulations of the motions of each particle are equivalent to a succession of contractions and dilatations; although each of these is next to infinitely small; their number is also next to infinitely great, and therefore the total effect may be sensible.

We have hitherto supposed that the absolute velocity of the particles was not diminished: this we did, having assumed that the interval of each undulation of the sides was without inequalities. But this was gratuitous: it was also gratuitous that the sides were only undulated. We have no reason for excluding angular asperities. These will produce, and most certainly often produce, real diminutions in the velocity of the contiguous particles; and this must extend to the very axis of the canal, and produce a diminution of the sum total of motion: and in order to preserve the same sensible progressive motion, a greater force must be employed. This is all that can be meant by saying that there is a resistance to the motion of air through long pipes.

There remains another cause of diminution, viz. the want of perfect fluidity, whether arising from the dissemination of solid particles in a real fluid, or from the viscidity of the fluid. We shall not insist on this at present, because it cannot be shown to obtain in air, at least in any case which deserves consideration. It seems of no importance to determine the motion of air hurrying along with it soot or dust. The effect of fogs on a particular modification of the motion of air will be considered under the article SOUND. What has been said on this subject is sufficient for our purpose, as explaining the prodigious and unexpected obstruction to the passage of air through long and narrow pipes. We are able to collect an important maxim from it, viz. that all pipes of communication should be made as wide as circumstances will permit: for it is plain that the obstruction depends on the internal surface, and the force to overcome it must be in proportion to the mass of matter which is in motion. The first increases as the diameter of the pipe, and the last as the square. The obstruction must therefore bear a greater proportion to the whole motion in a small pipe than in a large one.

It were very desirable to know the law by which the retardation extends from the axis to the sides of the canal, and the proportion which subsists between the lengths of canal and the forces necessary for overcoming the obstructions when the velocity is given; as also, whether the proportion of the obstruction to the whole motion varies with the velocity: but all this is unknown. It does not, however, seem a desperate case in air: we know pretty distinctly the law of action among its particles, viz. that their mutual repulsions are inversely as their distances. This promises to enable us to trace the progress of undulation from the sides of the canal to the axis.

We can see that the retardations will not increase so fast as the square of the velocity. Were the fluid incompressible, so that the undulatory path of a particle were invariable, the deflecting forces by which each individual particle is made to describe its undulating path would be precisely such as arise from the path itself and the motion in it; for each particle would be in the situation of a body moving along a fixed path. But in a very compressible fluid, such as air, each particle may be considered as a solitary body, actuated by a projectile and a transverse force, arising from the action of the adjoining particles. Its motion must depend on the adjustment of these forces, in the same manner as the elliptical motion of a planet depends on the adjustment of the force of projection, with a gravitation inversely proportional to the square of the distance from the focus. The transverse force in the present case has its origin in the pressure on the air which is propelling it along the pipe: this, by squeezing the particles together, brings their mutual repulsion into action. Now it is the property of a perfect fluid, that a pressure exerted on any part of it is propagated equally through the whole fluid; therefore the transverse forces which are excited by this pressure are proportional to the pressure itself: and we know that the pressures exerted on the surface of a fluid, so as to expel it through any orifice, or along any canal, are proportional to the squares of the velocities which they produce. Therefore, in every point of the undulatory motion of any particle, the transverse force by which it is deflected into a curve is proportional to the square of its velocity. When this is the case, a body would continue to describe the same curve as before; but, by the very compression, the curvatures are increased, supposing them to remain similar. This would require an increase of the transverse forces; but this is not to be found; therefore the particle will not describe a similar curve, but one which is less incurvated in all its parts; consequently the progressive velocity of the whole, which is the only thing perceivable by us, will not be so much diminished; that is, the obstructions will not increase so fast as they would otherwise do, or as the squares of the velocities.

This reasoning is equally applicable to all fluids, and is abundantly confirmed by experiments in hydraulics, as we shall see when considering the motion of rivers. We have taken this opportunity of delivering our notions on this subject: because, as we have often said, it is in the avowed discrete constitution of air that we see most distinctly the operation of those natural powers which constitute fluidity in general.

We would beg leave to mention a form of experiment for discovering the law of retardation with considerable accuracy. Experiments have been made on pipes and canals. M. Bossut, in his *Hydrodynamique*, has given a very beautiful set made on pipes of an inch and two inches diameter, and 200 feet long: but although these experiments are very instructive, they do not give us any rule by which we can extend the result to pipes of greater length and different diameters.

Let a smooth cylinder be set upright in a very large vessel or pond, and be moveable round its axis: let it be turned round by means of a wheel and pulley, with an uniform motion and determined velocity. It will exert the same force on the contiguous water which would be exerted on it by water turning round it with the same velocity: and as this water would have its motion gradually retarded by the fixed cylinder, so the moving cylinder will gradually communicate motion to the surrounding water. We should observe the water gradually dragged round by it;

and the vortex would extend further and further from it as the motion is continued, and the velocities of the parts of the vortex will be less and less as we recede from the axis. Now, we apprehend, that when a point of the surface of the cylinder has moved over 200 feet, the *motion* of the water at different distances from it will be similar and proportional to, if not precisely the same with, the *retardations* of water flowing 200 feet at the same distance from the side of a canal: at any rate, the two are susceptible of an accurate comparison, and the law of retardation may be accurately deduced from observations made on the motions of this vortex.

Air in motion is a very familiar object of observation; and it is interesting. In all languages it has got a name; we call it wind: and it is only upon reflection that we consider air as wind in a quiescent state. Many persons hardly know what is meant when air is mentioned; but they cannot refuse that the blast from a bellows is the expulsion of what they contained; and thus they learn that wind is air in motion.

It is of consequence to know the velocity of wind; but no good and unexceptionable method has been contrived for this purpose. The best seems to be by measuring the space passed over by the shadow of a cloud; but this is extremely fallacious. In the first place, it is certain, that although we suppose that the cloud has the velocity of the air in which it is carried along, this is not an exact measure of the current on the surface of the earth; we may be almost certain that it is greater: for air, like all other fluids, is retarded by the sides and bottom of the channel in which it moves. But, in the next place, it is very gratuitous to suppose, that the velocity of the cloud is the velocity of the stratum of air between the cloud and the earth; we are almost certain that it is not. It is abundantly proved by Dr Hutton of Edinburgh, that clouds are always formed when two parcels of air of different temperatures mix together, each containing a proper quantity of vapour in the state of chemical solution. We know that different strata of air will frequently flow in different directions for a long time. In 1781, while a great fleet rendezvoused in Leith Roads during the Dutch war, there was a brisk easterly wind for about five weeks; and, during the last fortnight of this period, there was a brisk westerly current at the height of about $\frac{3}{4}$ of a mile. This was distinctly indicated by frequent fleecy clouds at a great distance above a lower stratum of these clouds, which were driving all this time from the eastward. A gentleman who was at the siege of Quebec in 1759, informed us, that one day while there blew a gale from the west, so hard that the ships at anchor in the river were obliged to strike their topmasts, and it was with the utmost difficulty that some well-manned boats could row against it, carrying some artillery stores to a post above the town, several shells were thrown from the town to destroy the boats: one of the shells burst in the air near the top of its flight, which was about half a mile high. The smoke of this bomb remained in the same spot for above a quarter of an hour, like a great round ball, and gradually dissipated by diffusion, without removing many yards from its place. When, therefore, two strata of air come from different quarters, and one of them flows over the other, it will be only in the contiguous surfaces that a precipitation of vapour will be made. This will form a thin fleecy cloud; and it will have a velocity and direction which neither belongs to the upper nor to the lower stratum of air which produced it. Should one of these strata come from the east and the other from the west with equal velocities, the cloud formed between them will have no motion at all; should one come from the east, and the other from the north, the cloud will move from the north-east with a greater velocity than either of the strata. So uncertain then is the information given by the clouds either of the velocity or the direction of the wind. A thick smoke from a furnace will give us a much less equivocal measure: and this, combined with the effects of the wind in impelling bodies, or deflecting a loaded plane from the perpendicular, or other effects of this kind, may give us measures of the different currents of wind with a precision sufficient for all practical uses.

The celebrated engineer Mr John Smeaton has given, in the 51st volume of the Philosophical Transactions, the velocities of wind corresponding to the usual denominations in our language. These are founded on a great number of observations made by himself in the course of his practice in erecting wind-mills. They are contained in the following table:

Miles per hour.	Feet per second.	Names.
1	1,47	
2	2,93	Light airs.
3	4,40	
4	5,87	Breeze.
5	7,33	
10	14,67	Brisk gale.
15	22,	
20	29,34	Fresh gale.
25	36,67	
30	44,01	Strong gale.
35	51,34	
40	58,68	Hard gale.
45	66,01	
50	73,35	Storm.
60	88,02	
80	117,36	Hurricane, tearing up trees, overturning buildings, &c.
100	146,70	

See also some valuable experiments by him on this subject, Philosophical Transactions 1760 and 1761.

One of the most ingenious and convenient methods for measuring the velocity of the wind is to employ its pressure in supporting a column of water, in the same way as Mr Pitot measures the velocity of a current of water. We believe that it was first proposed by Dr James Lynd of Windsor, a gentleman eminent for his great knowledge in all the branches of natural science, and for his ingenuity in every matter of experiment or practical application.

His anemometer (as these instruments are called) consists of a glass tube of the form ABCD (Fig. 84.) open at both ends, and having the branch AB at right angles to the branch CD. This tube contains a few inches of water or any fluid (the lighter the better); it is held with the part CD upright, and AB horizontal and in the direction of the wind; that is, with the mouth A fronting the wind.

The wind acts in the way of pressure on the air in AB, compresses it, and causes it to press on the surface of the liquor; forcing it down to F, while it rises to E in the other leg. The velocity of the wind is concluded from the difference E*f* between the heights of the liquor in the legs. As the wind does not generally blow with uniform velocity, the liquor is apt to dance in the tube, and render the observation difficult and uncertain: to remedy this, it is proper to contract very much the communication at C between the two legs. If the tube has half an inch of diameter (and it should not have less), a hole of $\frac{1}{10}$ of an inch is large enough; indeed the hole can hardly be too small, nor the tubes too large.

This instrument is extremely ingenious, and will undoubtedly give the proportions of the velocities of different currents with the greatest precision; for in whatever way the pressure of wind is produced by its motion, we are certain that the different pressures are as the squares of the velocities: if, therefore, we can obtain one certain measure of the velocity of the wind, and observe the degree to which the pressure produced by it raises the liquor, we can at all other times observe the pressures and compute the velocities from them, making proper allowances for the temperature and the height of the mercury in the barometer; because the velocity will be in the subduplicate ratio of the density of the air inversely when the pressure is the same.

It is usually concluded, that the velocity of the wind is that which would be acquired by falling from a height which is to E*f* as the weight of water is to that of an equal bulk of air. Thus, supposing air to be 840 times lighter than water, and that E*f* is $\frac{9}{10}$ of an inch, the velocity will be about 63 feet *per* second, which is that of a very hard gale, approaching to a storm. Hence we see by the bye, that the scale of this instrument is extremely short, and that it would be a great improvement of it to make the leg CD not perpendicular, but very much sloping; or perhaps the following form of the instrument will give it all the perfection of which it is capable. Let the horizontal branch AB (Fig. 85.) be contracted at B, and continued horizontally for several inches BG of a much smaller bore, and then turned down for two or three inches GC, and then upwards with a wide bore. To use the instrument, hold it with the part DC perpendicular; and (having sheltered the mouth A from the wind) pour in water at D till it advances along GB to the point B, which is made the beginning of the scale; the water in the upright branch standing at *f* in the same horizontal line with BG. Now, turn the mouth A to the wind; the air in AB will be compressed, and will force the water along BG to F, and cause it to rise from *f* to E; and the range *f*E will be to the range BF on the scale as the section of the tube BG to that of CD. Thus, if the width of DC be ½ an inch, and that of BG $\frac{1}{10}$, we shall have 25 inches in the scale for one inch of real pressure E*f*.

But it has not been demonstrated in a very satisfactory manner, that the velocity of the wind is that acquired by falling through the height of a column of air whose weight is equal to that of the column of water E*f*. Experiments made with Pitot's tube in currents of water show that several corrections are necessary for concluding the velocity of the current from the elevations in the tube: these corrections may however be made, and safely applied to the present case; and then the instrument will enable us to conclude the velocity of the wind immediately, without any fundamental comparison of the elevation, with a velocity actually determined upon other principles. The chief use which we have for this information is in our employment of wind as an impelling power, by which we can actuate machinery or navigate ships. These are very important applications of pneumatical doctrines, and merit a particular consideration; and this naturally brings us to the last part of our subject, viz. the consideration of the impulse of air on bodies exposed to its action, and the resistance which it opposes to the passage of bodies through it.

This is a subject of the greatest importance; being the foundation of that art which has done the greatest honour to the ingenuity of man, and the greatest service to human society, by connecting together the most distant inhabitants of this globe, and making a communication of benefits which would otherwise have been impossible; we mean the art of Navigation or Seamanship. Of all the machines which human art has constructed, a ship is not only the greatest and most magnificent, but also the most ingenious and intricate; and the clever seaman possesses a knowledge founded on the most difficult and abstruse doctrines of mechanics. The seaman probably cannot give any account of his own science; and he possesses it rather by a kind of intuition than by any process of reasoning: but the success and efficacy of all the mechanism of this complicated engine, and the propriety of all the manœuvres which the seaman practises, depend on the invariable laws of mechanics; and a thorough knowledge of these would enable an intelligent person not only to understand the machine and the manner of working it, but to improve both.

Unfortunately this is a subject of very great difficulty; and although it has employed the genius of Newton, and he has considered it with great care, and his followers have added more to his labours on this subject than on any other, it still remains in a very imperfect state.

A minute discussion of this subject cannot therefore be expected in a work like this; we must content ourselves with such a general statement of the most approved doctrine on the subject as shall enable our readers to conceive it distinctly, and judge with intelligence and confidence of the practical deductions which may be made from it.

It is evidently a branch of the general theory of the impulse and resistance of fluids, which should have been treated of under the article HYDRAULICS, but was then deferred till the mechanical properties of compressible fluids should also be considered. It was thought very reasonable to suppose that the circumstances of elasticity would introduce the same changes in the impulse and resistance of fluids that it does in solid bodies. It would greatly divert the attention from the distinctive properties of air, if we should in this place enter on this subject, which is both extensive and difficult. We reckon it better therefore to take the whole together: this we shall do under the article **Resistance of Fluids**, and confine ourselves at present to what relates to the impulse and resistance of air alone; anticipating a few of the general propositions of that theory, but without demonstration, in order to understand the applications which may be made of it.

Suppose then a plane surface, of which *a* C (Fig. 86.) is the section, exposed to the action of a stream of wind blowing in the direction QC, perpendicular to *a* C. The motion of the wind will be obstructed, and the surface *a* C pressed forward. And as all impulse or pressure is exerted in a direction perpendicular to the surface, and is resisted in the opposite direction, the surface will be impelled in the direction CD, the continuation of QC. And as the mutual actions of bodies depend on their relative motions, the force acting on the surface *a* C will be the same, if we shall suppose the air at rest, and the surface moving equally swift in the opposite direction. The resistance of the air to the motion of the body will be equal to the impulse of the air in the former case. Thus resistance and impulse are equal and contrary.

If the air be moving twice as fast, its particles will give a double impulse; but in this case a double number of particles will exert their impulse in the same time: the impulse will therefore be fourfold, and in general it will be as the square of the velocity: or if the air and body be both in motion, the impulse and resistance will be proportional to the square of the relative velocity.

This is the first proposition on the subject, and it appears very consonant to reason. There will therefore be some analogy between the force of the air's impulse or the resistance of a body, and the weight of a column of air incumbent on the surface: for it is a principle in the action of fluids, that the heights of the columns of fluid are as the squares of the velocities which their pressures produce. Accordingly the second proposition is, that the absolute impulse of a stream of air, blowing perpendicularly on any surface, is equal to the weight of a column of air which has that surface for its base, and for its height the space through which a body must fall in order to acquire the velocity of the air.

Thirdly, Suppose the surface AC equal to *a* C no longer to be perpendicular to the stream of air, but inclined to it in the angle ACD, which we shall call the *angle of incidence*; then, by the resolution of forces, it follows, that the action of each particle is diminished in the proportion of radius to the sine of the angle of incidence, or of AC to AL, AL being perpendicular to CD.

Again: Draw AK parallel to CD. It is plain that no air lying farther from CD than KA is will strike the plane. The quantity of impulse therefore is diminished still farther in the proportion of *a* C to KC, or of AC to AL. Therefore, on the whole, the absolute impulse is diminished in the proportion of AC^2 to AL^2: hence the proposition, that the impulse and resistance of a given surface are in the proportion of the square of the sine of the angle of incidence.

Fourthly, This impulse is in the direction PL, perpendicular to the impelled surface, and the surface tends to move in this direction: but suppose it moveable only in some other direction PO, or that it is in the direction PO that we wish to employ this impulse, its action is therefore oblique; and if we wish to know the intensity of the impulse in this direction, it must be diminished still farther in the proportion of radius to the cosine of the angle LPO or sine of CPO. Hence the general proposition: The *effective impulse is as the surface, as the square of the velocity of the wind, as the square of the sine of the angle of incidence, and as the sine of obliquity jointly*, which we may express by the symbol $R = S.V^2. sin^2. I. sin. O$; and as the impulse depends on the density of the impelling fluid, we may take in every circumstance by the equation $R = S{\cdot}D{\cdot}V^2. sin.^2 I{\cdot} sin. O$. If the impulse be estimated in the direction of the stream, the angle of obliquity ACD is the same with the angle of incidence, and the impulse in this direction is as the surface, as the square of the velocity, and as the cube of the angle of incidence jointly.

It evidently follows from these premises, that if ACA′ be a wedge, of which the base AA′ is perpendicular to the wind, and the angle ACA′ bisected by its direction, the direct or perpendicular impulse on the base is to the oblique impulse on the sides as radius to the square of the sine of half the angle ACA′.

The same must be affirmed of a pyramid or cone ACA′, of which the axis is in the direction of the wind.

If ACA′ (Fig. 87.) represent the section of a solid produced by the revolution of a curve line APC round the axis CD, which lies in the direction of the wind, the impulse on this body may be compared with the direct impulse on its base, or the resistance to the motion of this body through the air may be compared with the direct resistance of its base, by resolving its surface into elementary planes P*p*, which are coincident with a tangent plane PR, and comparing the impulse on P*p* with the direct impulse on the corresponding part K*k* of the base.

In this way it follows that the impulse on a sphere is one half of the impulse on its great circle, or on the base of a cylinder of equal diameter.

We shall conclude this sketch of the doctrine with a very important proposition to determine the most advantageous position of a plane surface, when required to move in one direction while it is impelled by the wind blowing in a different direction. Thus,

Let AB (Fig. 88.) be the sail of a ship, CA the direction in which the wind blows, and AD the line of the ship's course. It is required to place the yard AC in such a position that the impulse of the wind upon the sail may have the greatest effect possible in impelling the ship along AD.

Let AB, A *b*, be two positions of the sail very near the best position, but on opposite sides of it. Draw BE, *b e* perpendicular to CA, and BF, *b f*, perpendicular to AD, calling AB radius; it is evident that BE, BF, are the sines of impulse and obliquity, and that the effective impulse is $BE^2 \times BF$, or $be^2 \times bf$. This must be a maximum.

Let the points B, *b*, continually approach and ultimately coincide; the chord *b* B will ultimately coincide with a straight line CBD touching the circle in B; the triangles CBE, *c b e* are similar, as also the triangles DBF, D *b f*: therefore $BE^2 : be^2 = BC^2 : bc^2$, and $BF : bf = BD : bD$; and $BE^2 \times BF : be^2 \times bf = CB^2 \times BD : cb^2 \times bD$. Therefore when AB is in the best position, so that $BE^2 \times BF$ is greater than $be^2 \times bf$, we shall have $CB \times BD$ greater than $Cb^2 \times bD$, or $cB^2 \times BD$ is also a maximum. This we know to be the case when $CB = 2\,BD$; therefore the sail must be so placed that the tangent of the angle of incidence shall be double of the tangent of the angle of the sail and keel.

In a common wind-mill the angle CAD is necessarily a right angle; for the sail moves in a circle to which the wind is perpendicular: therefore the best angle of the sail and axle will be 54°.44′ nearly.

Such is the theory of the resistance and impulse of the air. It is extremely simple and of easy application. In all physical theories there are assumptions which depend on other principles, and those on the judgment of the naturalist; so that it is always proper to confront the theory with experiment. There are even circumstances in the present case which have not been attended to in the theory. When a stream of air is obstructed by a solid body, or when a solid body moves along in air, the air is condensed before it and rarefied behind. There is therefore a pressure on the anterior parts arising from this want of equilibrium in the elasticity of the air. This must be superadded to the force arising from the impetus or inertia of the air. We cannot tell with precision what may be the amount of this condensation; it depends on the velocity with which any condensation diffuses itself.

Also, if the motion be so rapid that the pressure of the atmosphere cannot make the air immediately occupy the place quitted by the body, it will sustain this pressure on its forepart to be added to the other forces.

Experiments on this subject are by no means numerous; at least such experiments as can be depended on for the foundation of any practical application. The first that have this character are those published by Mr Robins, in 1742, in his Treatise on Gunnery. They were repeated with some additions by the Chevalier Borda, and some account of them published in the Memoirs of the Academy of Sciences in 1763. In the Philosophical Transactions of the Royal Society of London, vol. LXXIII. there are some experiments of the same kind on a larger scale by Mr Edgeworth. These were all made in the way described in our account of Mr Robins's improvements in gunnery. Bodies were made to move with determined velocities, and the resistances were measured by weights.

In all these experiments the resistances were found very exactly in the proportion of the squares of the velocities; but they were found considerably greater than the weight of the column of air, whose height would produce the velocity in a falling body. Mr Robins's experiments on a square of 16 inches, describing 25,2 feet per second, indicate the resistance to be to this weight nearly as 4 to 3.

Borda's experiments on the same surface state the disproportion still greater.

The resistances are found not to be in the proportion of the surfaces, but increase considerably faster. Surfaces of 9, 16, 36, and 81 inches, moving with one velocity, had resistances in the proportion of 9, 17¼, 40⅝, and 104⅘.

Now as this deviation from the proportion of the surfaces increases with great regularity, it is most probable that it continues to increase in surfaces of still greater extent; and these are the most generally to be met with in practice in the action of wind on ships and mills.

Borda's experiments on 81 inches show that the impulse of wind moving one foot per second is about $\frac{1}{500}$ of a pound on a square foot. Therefore to find the impulse on a foot corresponding to any velocity, divide the square of the velocity by 500, and we obtain the impulse in pounds. Mr Rouse of Leicestershire made many experiments, which are mentioned with great approbation by Mr Smeaton. His great sagacity and experience in the erection of windmills oblige us to pay a considerable deference to his judgment. These experiments confirm our opinion, that the impulses increase faster than the surfaces. The following table was calculated from Mr Rouse's observations, and may be considered as pretty near the truth.

Velocity in Feet.	Impulse on a Foot in Pounds.	Velocity in Feet.	Impulse on a Foot in Pounds.
0	0,000	80	14,638
10	0,229	90	18,526
20	0,915	100	22,872
30	2,059	110	27,675
40	3,660	120	32,926
50	5,718	130	38,654
60	8,234	140	44,830
70	11,207	150	51,462

If we multiply the square of the velocity in feet by 16, the product will be the impulse or resistance on a square foot in grains, according to Mr Rouse's numbers.

The greatest deviation from the theory occurs in the oblique impulses. Mr Robins compared the resistance of a wedge, whose angle was 90°, with the resistance of its base; and instead of finding it less in the proportion of $\sqrt{2}$ to 1, as determined by the theory, he found it greater in the proportion of 55 to 68 nearly; and when he formed the body into a pyramid, of which the sides had the same surface and the same inclination as the sides of the wedge, the resistance of the base and face were now as 55 to 39 nearly: so that here the same surface with the same inclination had its resistance reduced from 68 to 39 by being put into this form. Similar deviations occur in the experiments of the Chevalier Borda; and it may be collected from both, that the resistances diminish more nearly in the proportion of the sines of incidence than in the proportion of the squares of those sines.

The irregularity in the resistance of curved surfaces is as great as in plane surfaces. In general, the theory gives the oblique impulses on plane surfaces much too small, and the impulses on curved surfaces too great. The resistance of a sphere does not exceed the fourth part of the resistance of its great circle, instead of being its half; but the anomaly is such as to leave hardly any room for calculation. It would be very desirable to have the experiments on this subject repeated in a greater variety of cases, and on larger surfaces, so that the errors of the experiments may be of less consequence. Till this matter be reduced to some rule, the art of working ships must remain very imperfect, as must also the construction of wind-mills.

The case in which we are most interested in the knowledge of the resistance of the air is the motion of bullets and shells. Writers on artillery have long been sensible of the great effect of the air's resistance. It seems to have been this consideration that chiefly engaged Sir Isaac Newton to consider the motions of bodies in a resisting medium.

A proposition or two would have sufficed for showing the incompatibility of the planetary motions with the supposition that the celestial spaces were filled with a fluid matter; but he has with great solicitude considered the motion of a body projected on the surface of the earth, and its deviation from the parabolic track assigned by Galileo. He has bestowed more pains on this problem than any other in his whole work; and his investigation has pointed out almost all the improvements which have been made in the application of mathematical knowledge to the study of nature. Nowhere does his sagacity and fertility of resource appear in so strong a light as in the second book of the *Principia*, which is almost wholly occupied by this problem. The celebrated mathematician John Bernouilli engaged in it as the finest opportunity of displaying his superiority. A mistake committed by Newton in his attempt to a solution was matter of triumph to him; and the whole of his performance, though a piece of elegant and elaborate geometry, is greatly hurt by his continually bringing this mistake (which is a mere trifle) into view. The difficulty of the subject is so great, that subsequent mathematicians seem to have kept aloof from it; and it has been entirely overlooked by the many voluminous writers who have treated professedly on military projectiles. They have spoken indeed of the resistance of the air as affecting the flight of shot, but have saved themselves from the task of investigating this effect (a task to which they were unequal), by supposing that it was not so great as to render their theories and practical deductions very erroneous. Mr Robins was the first who seriously examined the subject. He showed, that even the Newtonian theory (which had been corrected, but not in the smallest degree improved or extended in its principles) was sufficient to show that the path of a cannon-ball could not resemble a parabola. Even this theory showed that the resistance was more than eight times the weight of the ball, and should produce a greater deviation from the parabola than the parabola deviated from a straight line.

This simple but singular observation was a strong proof how faulty the professed writers on artillery had been, in rather amusing themselves with elegant but useless applications of easy geometry, than in endeavouring to give their readers any useful information. He added, that the difference between the ranges by the Newtonian theory and by experiment were so great, that the resistance of the air must be vastly superior to what that theory supposed. It was this which suggested to him the necessity of experiments to ascertain this point. We have seen the result of these experiments in moderate velocities; and that they were sufficient for calling the whole theory in question, or at least for rendering it useless. It became necessary therefore to settle every point by means of a direct experiment. Here was a great difficulty. How shall we measure either these great velocities which are observed in the motions of cannon-shot, or the resistances which these enormous velocities occasion? Mr Robins had the ingenuity to do both. The method which he took for measuring the velocity of a musket-ball was quite original; and it was susceptible of great accuracy. We have already given some account of it in vol. i. p. 194. Having gained this point, the other was not difficult. In the moderate velocities he had determined the resistances by the forces which balanced them, the weights which kept the resisted body in a state of uniform motion. In the great velocities, he proposed to determine the resistances by their immediate effects, by the retardations which they occasioned. This was to be done by first ascertaining the velocity of the ball, and then measuring its velocity after it had passed through a certain quantity of air. The difference of these velocities is the retardation, and the proper measure of the resistance; for, by the initial and final velocities of the ball, we learn the time which was employed in passing through this air with the medium velocity. In this time the air's resistance diminished the velocity by a certain quantity. Compare this with the velocity which a body projected directly upwards would lose in the same time by the resistance of gravity. The two forces must be in the proportion of their effects. Thus we learn the proportion of the resistance of the air to the weight of the ball. It is indeed true, that the time of passing through this space is not accurately had by taking the arithmetical medium of the initial and final velocities, nor does the resistance deduced from this calculation accurately correspond to this mean velocity; but both may be accurately found by the experiment by a very troublesome computation, as is shown in the 5th and 6th propositions of the second book of Newton's *Principia*. The difference between the quantities thus found and those deduced from the simple process is quite trifling, and far within the limits of accuracy attainable in experiments of this kind; it may therefore be safely neglected.

Mr Robins made many experiments on this subject; but unfortunately he has published only a very few, such as were sufficient for ascertaining the point he had in view. He intended a regular work on the subject, in which the gradual variations of resistance corresponding to different velocities should all be determined by experiment: but he was then newly engaged in an important and laborious employment, as chief engineer to the East India Company, in whose service he went out to India, where he died in less than two years. It is to be regretted that no person has prosecuted these experiments. It would be neither laborious nor difficult, and would add more to the improvement of artillery than any thing that has been done since Mr Robins's death, if we except the prosecution of his experiments on the initial velocities of cannon-shot by Dr Charles Hutton, royal professor at the Woolwich Academy. It is to be hoped that this gentleman, after having with such effect and success extended Mr Robins's experiments on the initial velocities of musket-shot to cannon, will take up this other subject, and thus give the art of artillery all the scientific foundations which it can receive in the present state of our mathematical knowledge. Till then we must content ourselves with the practical rules which Robins has deduced from his own experiments. As he has not given us the mode of deduction, we must compare the results with experiments. He has indeed given a very extensive comparison with the numerous experiments made both in Britain and on the continent; and the agreement is very great. His learned commentator Euler has been at no pains to investigate these rules, and has employed himself chiefly in detecting errors, most of which are supposed, because he takes for a finished work what Mr Robins only gives to the public as a hasty but useful sketch of a new and very difficult branch of science.

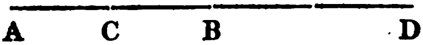
The general result of Robins's experiments on the retardation of musket-shot is, that although in moderate velocities, the resistance is so nearly in the duplicate proportion of the velocities that we cannot observe any deviation, yet in velocities exceeding 200 feet per second the retardations increase faster, and the deviation from this rate increases rapidly with the velocity. He ascribes this to the causes already mentioned, viz. the condensation of the air before the ball and to the rarefaction behind, in consequence of the air not immediately occupying the space left by the bullet. This increase is so great, that if the resistance to a ball moving with the velocity of 1700 feet in a second be computed on the supposition that the resistance observed in moderate velocities is increased in the duplicate ratio of the velocity, it will be found hardly one-third part of its real quantity. He found, for instance, that a ball moving through 1670 feet in a second lost about 125 feet per second of its velocity in passing through 50 feet of air.

This it must have done in the $\frac{1}{32}$ of a second, in which time it would have lost one foot if projected directly upwards; from which it appears that the resistance was about 125 times its weight, and more than three times greater than if it had increased from the resistance in small velocities in the duplicate ratio of the velocities. He relates other experiments which show similar results.

But he also mentions a singular circumstance, that till the velocities exceed 1100 feet per second, the resistances increase pretty regularly, in a ratio exceeding the duplicate ratio of the velocities; but that in greater velocities the resistances become suddenly triple of what they would have been, even according to this law of increase. He thinks this explicable by the vacuum which is then left behind the ball, it being well known that air rushes into a vacuum with the velocity of 1132 feet per second nearly. Mr Euler controverts this conclusion, as inconsistent with that gradation which is observed in all the operations of nature; and says, that although the vacuum is not produced in smaller velocities than this, the air behind the ball must be so rare (the space being but imperfectly filled), that the pressure on the anterior part of the ball must gradually approximate to that pressure which an absolute vacuum would produce; but this is like his other criticisms. Robins does no where assert that this sudden change of resistance happens in the transition of the velocity from 1132 feet to that of 1131 feet 11 inches or the like, but only that it is very sudden and very great. It may be strictly demonstrated, that such a change must happen in a narrow enough limit of velocities to justify the appellation of sudden: a similar fact may be observed in the motion of a solid through water. If it be gradually accelerated, the water will be found nearly to fill up its place, till the velocity arrives at a certain magnitude, corresponding to the immersion of the body in the water; and then the smallest augmentation of its motion immediately produces a void behind it, into which the water rushes in a violent manner, and is dashed into froth. A gentleman, who has had many opportunities for such observations, assures us, that when standing near the line of direction of a cannon discharging a ball with a large allotment of powder, so that the initial velocity certainly exceeded 1100 feet per second, he always observed a very sudden diminution of the noise which the bullet made during its passage. Although the ball was coming towards him, and therefore its noise, if equable, would be continually increasing, he observed that it was loudest at first. That this continued for a second or two, and suddenly diminished, changing to a sound which was not only weaker, but differed in kind, and gradually increased as the bullet approached him. He said, that the first noise was like the hissing of red-hot iron in water, and that the subsequent noise rather resembled a hazy whistling. Such a change of sound is a necessary consequence of the different agitation of the air in the two cases. We know also, that air rushing into a void, as when we break an exhausted bottle, makes a report like a musket.

Mr Robins's assertion therefore has every argument for its truth that the nature of the thing will admit. But we are not left to this vague reasoning: his experiments show us this diminution of resistance. It clearly appears from them, that in a velocity of 1700 feet the resistance is more than three times the resistance determined by the theory which he supposes the common one. When the velocity was 1065 feet, the actual resistance was $\frac{11}{7}$ of the theoretical; and when the velocity was 400 feet, the actual resistance was about $\frac{4}{5}$ of the theoretical. That he assumed a theory of resistance which gave them all too small, is of no consequence in the present argument.

Mr Robins, in summing up the results of his observations on this subject, gives a rule very easily remembered for computing the resistances to those very rapid motions. Let AB represent the velocity of 1700 feet per second, and



AC any other velocity. Make BD to AD as the resistance given by the ordinary theory to the resistance actually observed in the velocity 1700: then will CD be to AD as the resistance assigned by the ordinary theory to the velocity AC is to that which really corresponds to it.

To accommodate this to experiment, recollect that a sphere of the size of a 12-pound iron shot, moving 25 feet in a second, had a resistance of $\frac{1}{12}$ of a pound. Augment this in the ratio of 25^2 to 1700^2, and we obtain 210 nearly for the theoretical resistance to this velocity; but by comparing its diameter of $4\frac{1}{2}$ inches with $\frac{3}{4}$, the diameter of the leaden ball, which had a resistance of at least 11 pounds with this velocity, we conclude that the 12-pound shot would have had a resistance of 396 pounds: therefore BD : AD = 210 : 396, and AB : AD = 186 : 396; and AB being 1700, AD will be 3613.

Let AD = a, AC = x, and Let R be the resistance to a 12-pound iron shot moving one foot per second, and r the resistance (in pounds) wanted for the velocity x; we have $r = \mathrm{R}\,\frac{a\,x^2}{a - x}$. Mr Robins's experiments give $\mathrm{R} = \frac{1}{13750}$ very nearly. This gives $\mathrm{R}\,a = 0{,}263235$, which is nearly one-fourth. Thus our formula becomes $r = \frac{0{,}263235\,x^2}{3613 - x}$, or very nearly $\frac{x^2}{4(3613 - x)}$, falling short of the truth about $\frac{1}{20}$th part. The simplicity of the formula recommends it to our use, and when we increase its result $\frac{1}{20}$, it is incomparably nearer to the true result of the theory as corrected by Mr Robins than we can hope that the theory is to the actual resistance. We can easily see that

Mr Robins's correction is only a sagacious approximation. If we suppose the velocity 3613 feet, a very possible thing, the resistance by this formula is infinite, which cannot be. We may even suppose that the resistance given by the formula is near the truth only in such velocities as do not greatly exceed 1700 feet per second. No military projectile exceeds 2200, and it is great folly to make it so great, because it is reduced to 1700 almost in an instant, by the enormous resistance.

The resistance to other balls will be made by taking them in the duplicate ratio of the diameters.

It has been already observed, that the first mathematicians of Europe have lately employed themselves in improving this theory of the motion of bodies in a resisting medium; but their discussions are such as few artillerists can understand. The problem can only be solved by approximation, and this by the quadrature of very complicated curves. They have not been able therefore to deduce from them any practical rules of easy application, and have been obliged to compute tables suited to different cases. Of these performances, that of the Chevalier Borda, in the Memoirs of the Academy of Sciences in 1769, seems the best adapted to military readers, and the tables are undoubtedly of considerable use; but it is not too much to say, that the simple rules of Mr Robins are of as much service, and are more easily remembered: besides, it must be observed, that the nature of military service does not give room for the application of any very precise rule. The only advantage that we can derive from a perfect theory would be an improvement in the construction of pieces of ordnance, and a more judicious appropriation of certain velocities to certain purposes. The service of a gun or mortar must always be regulated by the eye.

There is another motion of which air and other elastic fluids are susceptible, viz. an internal vibration of their particles, or undulation, by which any extended portion of air is distributed into alternate parcels of condensed and rarefied air, which are continually changing their condition without changing their places. By this change the condensation which is produced in one part of the air is gradually transferred along the mass of air to the greatest distances in all directions. It is of importance to have some distinct conception of this motion. It is found to be by this means that distant bodies produce in us the sensation of sound. Sir Isaac Newton treated this subject with his accustomed ingenuity, and has given us a theory of it in the end of the second book of his *Principia*. This theory has been objected to with respect to the conduct of the argument, and other explanations have been given by the most eminent mathematicians. Though they appear to differ from Newton's, their results are precisely the same; but, on a close examination, they differ no more than John Bernouilli's theorem of centripetal forces differs from Newton's, viz. the one being expressed by geometry, and the other by literal analysis. The celebrated De la Grange reduces Newton's investigation to a tautological proposition or identical equation; but Mr Young of Trinity College, Dublin, has, by a different turn of expression, freed Newton's method from this objection.

But since Newton published this theory of aerial undulations, and of their propagation along the air, and since the theory has been so corrected and improved as to be received by the most accurate philosophers as a branch of natural philosophy susceptible of rigid demonstration, it has been freely resorted to by many writers on other parts of natural science, who did not profess to be mathematicians, but made use of it for explaining phenomena in their own line on the authority of the mathematicians themselves. Learning from them that this vibration, and the *quaquaversum* propagation of the pulses, were the necessary properties of an elastic fluid, and that the rapidity of this propagation had a certain assignable proportion to the elasticity and density of the fluid, they freely made use of these concessions, and have introduced elastic vibrating fluids into many facts, where others would suspect no such thing, and have attempted to explain by their means many abstruse phenomena of nature. Ethers are everywhere introduced, endued with great elasticity and tenuity. Vibrations and pulses are supposed in this ether, and these are offered as explanations. The doctrines of animal spirits and nervous fluids, and the whole mechanical system of Hartley, by which the operations of the soul are said to be explained, have their foundation in this theory of aerial undulations. If these fancied fluids, and their internal vibrations, really operate in the phenomena ascribed to them, any explanation that can be given of the phenomena from this principle must be nothing else than showing that the legitimate consequences of these undulations are similar to the phenomena; or, if we are no more able to see this last step than in the case of sound (which we know to be *one* consequence of the aerial undulations, although we cannot tell how), we must be able to point out, as in the case of sound, certain constant relations between the general laws of these undulations and the general laws of the phenomena. It is only in this way that we think ourselves entitled to say that the aerial undulations are causes, though not the only causes, of sound; and it is because there is no such relation, but, on the contrary, a total dissimilarity, to be observed between the laws of elastic undulations and the laws of the propagation of light, that we assert with confidence that ethereal undulations are not the causes of vision.

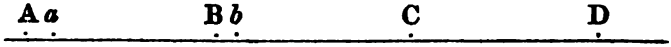
Explanations of this kind suppose, therefore, in the first place, that the philosopher who proposes them understands precisely the nature of these undulations; in the next place, that he makes his reader sensible of those circumstances of them which are concerned in the effect to be explained; and, in the third place, that he makes the reader understand how this circumstance of the vibrating fluid is connected with the phenomenon, either by showing it to be its mechanical cause, as when the philosopher explains the resounding of a musical chord to a flute or pipe which gave the same tone; or by showing that this circumstance of the undulation always accompanies the phenomenon, as when the philosopher shows that 233 vibrations of air in a second, in whatever manner or by whatever cause they are produced, always are followed by the sensation of the tone C in the middle of the harpsichord.

But here we must observe, that, with the exception of Euler's unsuccessful attempt to explain the optical phenomena by the undulations of ether, we have met with no explanation of natural phenomena, by means of elastic and vibrating fluids, where the author has so much as attempted any one of these three things, so indispensably requisite in a logical explanation. They have talked of vibrations without describing them, or giving the reader the least notion of what kind they are; and in no instance that we can recollect have they showed how such vibrations could have any influence in the phenomenon. Indeed, by not describing with precision the undulations, they were freed from the task of showing them to be mechanical causes of the phenomenon; and when any of them show any analogy between the general laws of elastic undulations and the general laws of the phenomenon, the analogy is so vague, indistinct, or partial, that no person of common prudence would receive it as argument in any case in which he was much interested.

We think it our duty to remonstrate against this slovenly way of writing: we would even hold it up to reprobation. It has been chiefly on this faithless foundation that the blind vanity of men has raised that degrading system of opinions called MATERIALISM, by which the affections and faculties of the soul of man have been resolved into vibrations and pulses of ether.

We also think it our duty to give some account of this motion of elastic fluids. It must be such an account as shall be understood by those who are not mathematicians, because those only are in danger of being misled by the improper application of them. Mathematical discussion is, however, unavoidable in a subject purely mathematical; but we shall introduce nothing that may not be easily understood or confided in; and we trust that mathematical readers will excuse us for a mode of reasoning which appears to them lax and inelegant.

The first thing incumbent on us is to show how elastic fluids differ from the unelastic in the propagation of any agitation of their parts. When a long tube is filled with water, and any one part of it pushed out of its place, the whole is instantly moved like a solid mass. But this is not the case with air. If a door be suddenly shut, the window at the farther end of a long and close room will rattle; but some time will elapse between the shutting of the door and the motion of the window. If some light dust be lying on a braced drum, and another be violently beat at a little distance from it, an attentive observer will see the dust dance up from the parchment; but this will be at the instant he hears the sound of the stroke on the other drum, and a sensible time after the stroke. Many such familiar facts show that the agitation is gradually communicated along the air; and therefore that when one particle is agitated by any sensible motion, a finite time, however small, must elapse before the adjoining particle is agitated in the same manner. This would not be the case in water if water be perfectly incompressible. We think that this may be made intelligible with very little trouble.



Let A, B, C, D, &c. be a row of aerial particles, at such distances that their elasticity just balances the pressure of the atmosphere; and let us suppose (as is deducible from the observed density of air being proportional to the compressing force) that the elasticity of the particles, by which they keep each other at a distance, is as their distances inversely. Let us farther suppose that the particle A has been carried, with an uniform motion, to a by some external force. It is evident that B cannot remain in its present state; for being now nearer to a than to C, it is propelled towards C by the excess of the elasticity of A above the natural elasticity of C. Let E be the natural elasticity of the particles, or the force corresponding to the distance BC or BA, and let F be the force which impels B towards C, and let f be the force exerted by A when at a. We have

$$E : f = Ba : BC, = Ba : BA;$$
$$\text{and } E : f - E = Ba : BA - Ba = Ba : Aa;$$
$$\text{or } E : F = Ba : Aa.$$

Now, in Fig. 89. let ABC be the line joining three particles, to which draw FG, PH parallel, and IAF, HBG perpendicular. Take IF or HG to represent the elasticity corresponding to the distance AB. Let the particle A be supposed to have been carried with an uniform motion to a by some external force, and draw R a M perpendicular to RG, and make FI : RM = B a : BA. We shall then have FI : PM = Ba : Aa; and PM will represent the force with which the particle B is urged towards C. Suppose this construction to be made for every point of the line AB, and that a point M is thus determined for each of them, mathematicians know that all these points M lie in the curve of a hyperbola, of which FG and GH are the asymptotes. It is also known by the elements of mechanics, that since the motion of A along AB is uniform A a or IP may be taken to represent the time of describing A a; and that the area IPM represents the whole velocity which B has acquired in its motion towards C when A has come to *a*, the force urging B being always as the portion PM of the ordinate.

Take GX of any length in HG produced, and let GX represent the velocity which the uniform action of the natural elasticity IF could communicate to the particle B during the time that A would uniformly describe AB. Make GX to GY as the rectangle IFGH to the hyperbolic space IFRM, and draw YS cutting MR produced in S, and draw FX cutting MR in T. It is known to the mathematicians that the point S is in a curve line FS*s* called the *logarithmic curve*; of which the leading property is, that any line RS parallel to GX is to GX as the rectangle IFGH is to the hyperbolic space IFRM, and that FX touches the curve in F.

This being the case, it is plain, that because RT increases in the same proportion with FR, or with the rectangle IFRP, and RS increases in the proportion of the space IFRM, TS increases in the proportion of the space IPM. Therefore TS is proportional to the velocity of B when A has reached *a*, and RT is proportional to the velocity which the uniform action of the natural elasticity would communicate to B in the same time. Then since FT is as the time, and TS is as the velocity, the area FTS will be as the space described by B (urged by the variable force PM); while A, urged by the external force, describes A*a*; and the triangle FRT will represent the space which the uniform action of the natural elasticity would cause B to describe in the same time.

And thus it is plain that these three motions can be compared together: the uniform motion of the agitated particle A, the uniformly accelerated motion which the natural elasticity would communicate to B by its constant action, and the motion produced in B by the agitation of A. But this comparison, requiring the quadrature of the hyperbola and logarithmic curve, would lead us into most intricate and tedious computations. Of these we need only give the result, and make some other comparisons which are palpable.

Let A *a* be supposed indefinitely small in comparison of AB. The space described by A is therefore indefinitely small; but in this case we know that the ratio of the space FRT to the rectangle IFRP is indefinitely small. There is therefore no comparison between the agitation of A by the external force, and the agitation which natural elasticity would produce on a single particle in the same time, the last being incomparably smaller than the first. And this space FRT is incomparably greater than FTS; and therefore the space which B would describe by the uniform action of the natural elasticity is incomparably greater than what it would describe in consequence of the agitation of A.

From this reasoning we see evidently that A must be sensibly moved, or a finite or measurable time must elapse before B acquires a measurable motion. In like manner B must move during a measurable time before C acquires a measurable motion, &c.; and therefore the agitation of A is communicated to the distant particles in gradual succession.

By a farther comparison of these spaces we learn the time in which each succeeding particle acquires the very agitation of A. If the particles B and C only are considered, and the motion of C neglected, it will be found that B has acquired the motion of A a little before it has described $\frac{1}{3}$ of the space described by A; but if the motion of C be considered, the acceleration of B must be increased by the retreat of C, and B must describe a greater space in proportion to that described by A. By computation it appears, that when both B and C have acquired the velocity of A, B has described nearly $\frac{1}{2}$ of A's motion, and C more nearly $\frac{1}{3}$. Extending this to D, we shall find that D has described still more nearly $\frac{1}{4}$ of A's motion. And from the nature of the computation it appears that this approximation goes on rapidly: therefore, supposing it accurate from the very first particle, it follows from the equable motion of A, that each succeeding particle moves through an equal space in acquiring the motion of A.

The conclusion which we must draw from all this is, that when the agitation of A has been fully communicated to a particle at a *sensible* distance, the intervening particles, all moving forward with a common velocity, are equally compressed as to sense, except a very few of the first particles; and that this communication, or this propagation of the original agitation, goes on with an uniform velocity.

These computations need not be attended to by such as do not wish for an accurate knowledge of the precise agitation of each particle. It is enough for such readers to see clearly that time *must* escape between the agitation of A and that of a distant particle; and this is abundantly manifest from the incomparability (excuse the term) of the nascent rectangle IFRP with the nascent triangle FRT, and the incomparability of FRT with FTS.

What has now been shown of the communication of any sensible motion A *a* must hold equally with respect to any change of this motion. Therefore if a tremulous motion of a body, such as a spring or bell, should agitate the adjoining particle A by pushing it forward in the direction AB, and then allowing it to come back again in the direction BA, an agitation similar to this will take place in all the particles of the row one after the other. Now if this body vibrate according to the law of motion of a pendulum vibrating in a cycloid, the neighbouring particle of air *will of necessity* vibrate in the same manner; and then Newton's demonstration needs no apology. Its only deficiency was, that it *seemed* to prove that this *would be* the way in which every particle would of necessity vibrate; which is not true, for the successive parcels of air will be differently agitated according to the original agitation. Newton only wants to prove the uniform propagation of the agitations, and he selects that form which renders the proof easiest. He proves, in the most unexceptionable manner, that if the particles of a pulse of air are really moving like a cycloidal pendulum, the forces acting on each particle, in consequence of the compression and dilatation of the different parts of the pulse, are precisely such as are necessary for continuing this motion, and therefore no other forces are required. Then since each particle is in a certain part of its path, is moving in a certain direction, and with a certain velocity, and urged by a determined force, it *must* move in that very manner. The objection started by John Bernouilli against Newton's demonstration (in a single line) of the elliptical motion of a body urged by a force in the inverse duplicate ratio of the distance from the focus, is precisely the same with the objection against Newton's demonstration of the progress of aerial undulations, and is equally futile.

It must, however, be observed, that Newton's demonstration proceeds on the supposition that the linear agitations of a particle are incomparably smaller than the extent of an undulation. This is not strictly the case in any instance, and in many it is far from being true. In a pretty strong twang of a harpsichord wire, the agitation of a particle may be near the 50th part of the extent of the undulation. This must disturb the regularity of the motion, and cause the agitations in the remote undulations to differ from those in the first pulse. In the explosion of a cannon, the breaking of an exhausted bottle, and many instances which may be given, the agitations are still greater. The commentators on Newton's *Principia*, Le Sueur and Jacquier, have shown, and Euler more clearly, that when the original agitations are very violent, the particles of air will acquire a subordinate vibration compounded with the regular cycloidal vibration, and the progress of the pulses will be somewhat more rapid; but the intricacy of the calculus is so great, that they have not been able to determine with any tolerable precision what the change of velocity will be.

All this, however, is fully confirmed by experiment on sounds. The sound of a cannon at 10 or 20 miles distance does not in the least resemble its sound when near. In this case it is a loud instantaneous crack, to which we can assign no musical pitch: at a distance, it is a grave sound, of which we can tell the note; and it begins softly, swells to its greatest loudness, and then dies away growling. The same may be said of a clap of thunder, which we know to be a loud snap of still less duration. It is highly probable that the appreciable tone which those distant sounds afford are produced by the continuance of these subordinate vibrations which are added together and fortified in the successive pulses, though not perceptible in the first, in a way somewhat resembling the resonance of a musical chord. Newton's explanation gathers evidence therefore from this circumstance. And we must further observe, that all elastic bodies tremble or vibrate almost precisely as a pendulum swinging in a cycloid, unless their vibrations are uncommonly violent; in which case they are quickly reduced to a moderate quantity by the resistance of the air. The only very loud sounds which we can produce in this way are from great bells; and in these the utmost extent of the vibration is very small in comparison with the breadth of the pulse. The velocity of these sounds has not been compared with that of cannon, or perhaps it would be found less, and an objection against Newton's determination removed. He gives 960 feet per second, Experiment 1142.

But it is also very probable, that in the propagation through the air, the agitation gradually and rapidly approaches to this regular cycloidal form in the successive pulses, in the same way as we observe that whatever is the form of agitation in the middle of a smooth pond of water, the spreading circles are always of one gentle form without asperities. In like manner, into whatever form we throw a stretched cord by the twang which we give it, it almost immediately makes smooth undulations, keeping itself in the shape of an elongated trochoid. Of this last we can demonstrate the necessity, because the case is simple. In the wave, the investigation is next to impossible; but we see the fact. We may therefore presume it in air. And accordingly we know that any noise, however abrupt and jarring, near at hand, is smooth at a distance. Nothing is more rough and harsh than the scream of a heron; but at half a mile's distance it is soft. The ruffle of a drum is also smooth at a distance.

Fig. 90. shows the successive situations of the particles of a row. Each line of the figure shows the same particles marked with the same letters; the first particle *a* being supposed to be removed successively from its quiescent situation and back to it again. The mark × is put on that part of each line where the agitated particles are at their natural distances, and the air is of the natural density. The mark 1 is put where the air is most of all compressed, and : where it is most of all dilated; the curve line drawn through the lowest line of the figure is intended to represent the density in every point, by drawing ordinates to it from the straight line: the ordinates below the line indicate a rarity, and those above the line a density, greater than common.

It appears that when *a* has come back to its natural situation, the part of greatest density is between the particles *i* and *k*, and the greatest rarity between *c* and *d*.

We have only to add, that the velocity of this propagation depends on the elasticity and density of the fluid. If these vary in the same proportion, that is, if the fluid has its elasticity proportional to its density, the velocity will remain the same. If the elasticity or density alone be changed, the velocity of the undulations will change in the direct subduplicate ratio of the elasticity and the inverse subduplicate ratio of the density; for should the elasticity be quadrupled, the quantity of motion produced by it in any given time will be quadrupled. This will be the case if the velocity be doubled; for there would then be double the number of particles doubly agitated. Should the density be quadrupled, the elasticity remaining the same, the quantity of motion must remain the same. This will be the case if the velocity be reduced to one half; for this will propagate half the agitation to half the distance, which will communicate it to twice the number of particles, and the quantity of motion will remain the same. The same may be said of other proportions, and therefore $V = \frac{\sqrt{E}}{\sqrt{D}}$. Therefore a change in the barometer will not affect the velocity of the undulations in air, but they will be accelerated by heat, which diminishes its density, or increases its elasticity. The velocity of the pulses in inflammable air must be at least thrice as great, because its density is but one-tenth of that of air when the elasticity of both are the same.

Let us now attend a little to the propagation of aerial pulses as they really happen; for this hypothesis of a single row of particles is nowhere to be observed.

Suppose a sphere A, Fig. 91. filled with condensed air, and that the vessel which contains it is suddenly annihilated. The air must expand to its natural dimensions, suppose BCD. But it cannot do this without pressing aside the surrounding air. We have seen that in any single row of particles this cannot be at once diffused to a distance, but must produce a condensation in the air adjoining; which will be gradually propagated to a distance. Therefore this sphere BCD of the common density will form round it a shell, bounded by EFG, of condensed air. Suppose that at this instant the inner air BCD becomes solid. The shell of condensed air can expand only outwards. Let it expand till it is of the common density, occupying the shell HIK. This expansion, in like manner, must produce a shell of condensed air without it: at this instant let HIK become solid. The surrounding shell of condensed air can expand only outward, condensing another shell without it. It is plain that this must go on continually, and the central agitation will be gradually propagated to a distance in all directions. But, in this process, it is not the same numerical particles that go to a distance. Those of the original sphere go no further than BCD; those of the next shell go no further than HIK, &c. Farther, the expansion outwards of any particle will be more moderate as the diffusion advances; for the whole motion of each shell cannot exceed the original quantity of motion; and the number of particles in each successive shell increases as the surface, that is, as the square of the distance from the centre: therefore the agitation of the particles will decrease in the same ratio, or will be in the inverse duplicate ratio of the distance from the centre. Each successive shell, therefore, contains the same quantity of motion, and the successive agitations of the particles of any row out from the centre will not be equal to the original agitation, as happens in the solitary row. But this does not affect the velocity of the propagation, because all agitations are propagated equally fast.

We supposed the air A to become solid as soon as it acquired the common density; but this was to facilitate the conception of the diffusion. It does not stop at this bulk; for while it was denser it had a tendency to expand. Therefore each particle has attained this distance with an accelerated motion. It will, therefore, continue this motion like a pendulum that has passed the perpendicular, till it is brought to rest by the air without it; and it is now rarer than common air, and collapses again by the greater elasticity of the air without it. This outward air, therefore, in regaining its natural density, must expand both ways. It expands towards the centre, following the collapsing of the air within it; and it expands outwards, condensing the air beyond it. By expanding inwards, it will again condense the air within it, and this will again expand; a similar motion happens in all the outward shells; and thus there is propagated a succession of condensed and rarefied shells of air, which gradually swell to the greatest distance.

It may be demonstrated, that when the central air has for the second time acquired the natural density, it will be at rest, and be disturbed no more; and that this will happen to all the shells in succession. But the demonstration is much too intricate for this place; we must be contented with pointing out a fact perfectly analogous. When we drop a small pebble into water, we see it produce a series of circular waves, which go along the surface of smooth water to a great distance, becoming more and more gentle as they recede from the centre; and the middle, where the agitation was first produced, remains perfectly smooth, and this smoothness extends continually; that is, each wave, when brought to a level, remains at rest. Now these waves are produced and propagated by the depression and elevation made at the centre. The elevation tends to diffuse itself; and the force with which each particle of water is actuated is a force acting directly up and down, and is proportional to the elevation and depression of the particle. This hydrostatical pressure operates precisely in the same way as the condensation and rarefaction of the air; and the mathematical investigation of the propagation of the circular undulations on smooth water is similar in every step to that of the propagation of the spherical waves in still air. For this we appeal to Newton's *Principia*, or to Euler's *Opuscula*, where he gives a very beautiful investigation of the velocity of the aerial pulses; and to some memoirs of de la Grange in the collections of the academies of Berlin and Turin. These two last authors have made the investigation as simple as it seems possible, and have freed it from every objection which can be stated against the geometrical one of their great teacher Newton.

Having said this much on the similarity between the waves on water and the aerial undulations, we shall have recourse to them, as affording us a very sensible object to represent many affections of the other which it would be extremely difficult to explain. We neither see nor feel the aerial undulations; and they behoved, therefore, to be described very abstractedly and imperfectly. In the watery wave there is no permanent progressive motion of the water from the centre. Throw a small bit of cork on the surface, and it will be observed to popple up and down without the least motion outwards. In like manner, the particles of air are only agitated a very little outwards and inwards; which motion is communicated to the particles beyond them, while they themselves come to rest, unless agitated afresh; and this agitation of the particles is inconceivably small. Even the explosion of a cannon at no great distance will but gently agitate a feather, giving it a single impulse outwards, and immediately after another inwards or towards the cannon. When a harpsichord wire is forcibly twanged at a few feet distance, the agitation of the air is next to insensible. It is not, however, nothing; and it differs from that in a watery wave by being *really* outwards and inwards. In consequence of this, when the condensed shell reaches an elastic body, it impels it slightly. If its elasticity be such as to make it acquire the opposite shape at the instant that the next agitation and condensed shell of air touches it, its agitation will be doubled, and a third agitation will increase it, and so on, till it acquire the agitation competent to that of the shell of air which reaches it, and it is thrown into *sensible* vibration, and gives a sound extremely faint indeed, because the agitation which it acquires is that corresponding to a shell of air considerably removed from the original string. Hence it happens that a musical chord, pipe, or bell, will cause another to resound, whose vibrations are isochronous with its own; or if the vibrations of the one coincides with every second, or third, or fourth, &c. of the other; just as we can put a very heavy pendulum into sensible motion by giving it a gentle puff with the breath at every vibration, or at every second, third, or fourth, &c. A drum struck in the neighbourhood of another drum will agitate it *very sensibly*; for here the stroke depresses a very considerable surface, and produces an agitation of a considerable mass of air: it will even agitate the surface of stagnant water. The explosion of a cannon will even break a neighbouring window. The shell of condensed air which comes against the glass has a great surface and a great agitation: the best security in this case is to throw up the sash; this admits the condensed air into the room, which acts on the inside of the window, balancing part of the external impulse.

It is demonstrated in every elementary treatise of natural philosophy, that when a wave on water meets any plane obstacle, it is reflected by it from a centre equally removed behind the obstacle; that waves radiating from the focus of a parabola are reflected in waves perpendicular to its axis; that waves radiating from one focus of an ellipse are made to converge to the other focus, &c. &c. All this may be affirmed of the aerial undulations; that when part of a wave gets through a hole in the obstacle, it becomes the centre of a new series of waves; that waves bend round the extremities of an obstacle: all this happens in the aerial undulations. And, lastly, that when the surface of water is thrown into regular undulations by one agitation, another agitation in another place will produce other regular waves, which will cross the former without disturbing them in the smallest degree. The same thing happens in air; and experiments may be made on water which will illustrate in the most perfect manner many other affections of the aerial pulses, which we should otherwise conceive very imperfectly. We would recommend to our curious readers to make some of these experiments in a large vessel of milk. Take a long and narrow plate of lead, which, when set on the bottom of the vessel, will reach above the surface of the milk; bend this plate into a parabola, elliptical or other curve. Make the undulations by dropping milk on the focus from a small pipe, which will cause the agitations to succeed with rapidity, and then all that we have said will be most distinctly seen, and the experiment will be very amusing and instructive, especially to the musical reader.

We would now request all who make or read explanations of natural phenomena by means of vibrations of ethers, animal spirits, nervous fluids, &c. to fix their attention on the nature of the agitation in one of these undulations. Let him consider whether this can produce the phenomenon, acting as any matter must act, by impulse or by pressure. If he sees that it can produce the phenomenon, he will be able to point out the very motion it will produce, both in quantity and direction, in the same manner as Sir Isaac Newton has pointed out all the irregularities of the moon's motion produced by the disturbing force of the sun. If he cannot do this, he fails in giving the first evidence of a mechanical explanation by the action of an elastic vibrating fluid. Let him then try to point out some palpable connexion between the general phenomena of elastic undulations and the phenomenon in question; this would show an accompaniment to have at least some probability. It is thus only we learn that the undulations of air produce sound: we cannot tell how they affect the mechanism of the ear; but we see that the phenomena of sound always accompany them, and that certain modifications of the one are regularly accompanied by certain modifications of the other. If we cannot do this, we have derived neither explanation nor illustration from the elastic fluid. And, lastly, let him remember that even if he should be able to show the competency of this fluid to the production of the phenomenon, the whole is still an hypothesis, because we do not know that such a fluid exists.

We will venture to say, that whoever will proceed in this prudent manner will soon see the futility of most of the explanations of this kind which have been given. They are unfit for any but consummate mathematicians; for they alone really understand the mechanism of aerial undulations, and even they speak of them with hesitation as a thing but imperfectly understood. But even the unlearned in this science can see the incompatibility of the hypotheses with many things which they are brought to explain. To take an instance of the conveyance of sensation along the nerves; an elastic fluid is supposed to occupy them, and the undulations of this fluid are thought to be propagated along the nerves. Let us just think a little how the undulations would be conveyed along the surface of a canal which was completely filled up with reeds and bulrushes, or let us make the experiment on such a canal: we may rest assured that the undulations in the one case will resemble those in the other; and we may see that in the canal there will be no regular or sensible propagation of the waves.

Let these observations have their influence, along with others which we have made on other occasions, to wean our readers from this fashionable proneness to introduce invisible fluids and unknown vibrations into our physical discussions. They have done immense, and, we fear, irreparable, mischief in science; and there is but one phenomenon that has ever received any explanation by their means.

This may suffice for a loose and popular account of aerial undulations; and with it we conclude our account of the motion, impulse, and resistance of air.

We shall now explain a number of natural appearances, depending on its pressure and elasticity, appearances not sufficiently general, or too complicated for the purposes of argument, while we were employed in the investigation of these properties, but too important to be passed over in silence.

It is owing to the pressure of the atmosphere that two surfaces which accurately fit each other cohere with such force. This is a fact familiarly known to the glass-grinders, polishers of marble, &c. A large lens or speculum, ground on its tool till it becomes very smooth, requires more than any man's strength to separate it directly from the tool. If the surface is only a square inch, it will require 15 pounds to separate them perpendicularly, though a very moderate force will make them slide along each other. But this cohesion is not observed unless the surfaces are wetted or smeared with oil or grease; otherwise the air gets between them, and they separate without any trouble. That this cohesion is owing to the atmospheric pressure, is evident from the ease with which the plates may be separated in an exhausted receiver.

To the same cause we must ascribe the very strong adhesion of snails, periwinkles, limpets, and other univalve shells, to the rocks. The animal forms the rim of its shell, so as to fit the shape of the rock to which it intends to cling. It then fills its shell (if not already filled by its own body) with water. In this condition it is evident that we must act with a force equal to 15 pounds for every square inch of touching surface before we can detach it. This may be illustrated by filling a drinking glass to the brim with water; and having covered it with a piece of thin wet leather, whelm it on a table, and then try to pull it straight up; it will require a considerable force. But if we expose a snail adhering to a stone in the exhausted receiver, we shall see it drop off by its own weight. In the same manner do the remora, the polypus, the lamprey, and many other animals, adhere with such firmness. Boys frequently amuse themselves by pulling out large stones from the pavement by means of a circle of stiff wetted leather fastened to a string. It is owing to the same cause that the bivalve shell fishes keep themselves so firmly shut. We think the muscular force of an oyster prodigious, because it requires such force to open it; but if we grind off a bit of the convex shell, so as to make a hole in it, though without hurting the fish in the smallest degree, it opens with great ease, as it does also *in vacuo*.

The pressure of the air, operating in this way, contributes much to the cohesion of bodies, where we do not suspect its influence. The tenacity of our mortars and cements would frequently be ineffectual without this assistance.

It is owing to the pressure of the atmosphere that a cask will not run by the cock unless a hole be opened in some other part of the cask. If the cask is not quite full, some liquor indeed will run out, but it will stop as soon as the diminished elasticity of the air above the liquor is in equilibrio (together with the liquor) with the atmospheric pressure. In like manner, a teapot must have a small hole in its lid to ensure its pouring out the tea. If indeed the hole in the cask is of large dimensions, it will run without any other hole, because air will get in at the upper side of the hole while the liquor runs out by the lower part of it.

On the same principle depends the performance of an instrument used by the spirit-dealers for taking out a sample of their spirits. It consists of a long tin-plate tube AB (Fig. 74.) open atop at A, and ending in a small hole at B. The end B is dipped into the spirits, which rises into the tube; then the thumb is clapt on the mouth A, and the whole is lifted out of the cask. The spirit remains in it till the thumb be taken off; it is then allowed to run into a glass for examination.

It seems principally owing to the pressure of the air that frosts immediately occasion a scantiness of water in our fountains and wells. This is erroneously accounted for, by supposing that the water freezes in the bowels of the earth. But this is a great mistake: the most intense frost of a Siberian winter would not freeze the ground two feet deep; but a very moderate frost will consolidate the whole surface of a country, and make it impervious to the air; especially if the frost has been preceded by rain, which has soaked the surface. When this happens, the water which was filtering through the ground is all arrested and kept suspended in its capillary tubes by the pressure of the air, in the very same manner as the spirits are kept suspended in the instrument just now described by the thumb's shutting the hole A. A thaw melts the superficial ice, and allows the water to run in the same manner as the spirits run when the thumb is removed.

Common air is necessary for supporting the lives of most animals. If a small animal, such as a mouse or bird, be put under the receiver of an air-pump, and the air be exhausted, the animal will quickly be thrown into convulsions and fall down dead; if the air be immediately readmitted, the animal will sometimes revive, especially if the rarefaction has been briskly made, and has not been very great. We do not know that any breathing animal can bear the air to be reduced to $\frac{1}{4}$ of its ordinary density, nor even $\frac{1}{3}$; nor have we good evidence that an animal will ever recover if the rarefaction is pushed very far, although continued for a very short time.

But the mere presence of the air is by no means sufficient for preserving the life of the animal; for it is found, that an animal shut up in a vessel of air cannot live in it for any length of time. If a man be shut up in a box, containing a wine hogshead of air, he cannot live in it much above an hour, and long before this he will find his breathing very unsatisfactory and uneasy. A gallon of air will support him about a minute. A box EF (Fig. 75.) may be made, having a pipe AB inserted into its top, and fitted with a very light valve at B, opening upwards. This pipe sends off a lateral branch *d* D *d* C, which enters the box at the bottom, and is also fitted with a light valve at C opening upwards. If a person breathe through the pipe, keeping his nostrils shut, it is evident that the air which he expires will not enter the box by the hole B, nor return through the pipe CD *d*; and by this contrivance he will gradually employ the whole air of the box. With this apparatus experiments can be made without any risk or inconveniency, and the quantity of air necessary for a given time of easy breathing may be accurately ascertained.

How the air of our atmosphere produces this effect, is a question which does not belong to mechanical philosophy to investigate or determine. We can, however, affirm, that it is neither the pressure nor the elasticity of the air which is immediately concerned in maintaining the animal functions. We know that we can live and breathe with perfect freedom on the tops of the highest mountains. The valley of Quito in Peru, and the country round Gondar in Abyssinia, are so far elevated above the surface of the ocean, that the pressure and the elasticity of the air are one-third less than in the low countries; yet these are populous and healthy places. And, on the other hand, we know, that when an animal has breathed in any quantity of air for a certain time without renewal, it will not only be suffocated, but another animal put into this air will die immediately; and we do not find either the pressure or elasticity of the air remarkably diminished: it is indeed diminished, but by a very small quantity. Restoring the former pressure and elasticity has not the smallest tendency to prevent the death of the animal: for an animal will live no longer under a receiver that has its mouth inverted on water, than in one set upon the pump-plate covered with leather. Now, when the receiver is set on water, the pressure of the atmosphere acts completely on the included air, and preserves it in the same state of elasticity.

In short, it is known that the air which has already served to maintain the animal functions, has its chemical and alimentary properties completely changed, and is no longer fit for this purpose. So much of any mass of air as has really been thus employed is changed into what is called *fixed air* by Dr Black, or *carbonic acid* by the chemists of the Lavoisierian school. Any person may be convinced of this by breathing or blowing through a pipe immersed in lime water. Every expiration will produce white clouds on the water, till all the lime which it contains is precipitated in the form of pure chalk. In this case we know that the lime has combined with the fixed air.

The celebrated Dr Stephen Hales made many experiments with a view to clear the air from the noxious vapour which he supposed to be emitted from the lungs.

He made use of the apparatus which we have been just now mentioning; and he put several diaphragms *ff*, *ff*, &c. of thin woollen stuff into the box, and moistened them with various liquids. He found nothing so efficacious as a solution of potash. We now understand this perfectly. If the solution is not already saturated with fixed air, it will take it up as fast as it is produced, and thus will purify the air: a solution of caustic alkali therefore will have this effect till it is rendered quite mild.

These experiments have been repeated, and varied in many circumstances, in order to ascertain whether this fixed air was really emitted by the lungs, or whether the inspired air was in part changed into fixed air by its combination with some other substance. This is a question which comes properly in our way, and which the doctrines of pneumatics enable us to answer. If the fixed air be emitted in substance from the lungs, it does not appear how a renewal of the air into which it is emitted is necessary: for this does not hinder the subsequent emission; and the bulk of the air would be increased by breathing in it, viz. by the bulk of all the fixed air emitted; but, on the contrary, it is a little diminished. We must therefore adopt the other opinion; and the discoveries in modern chemistry enable us to give a pretty accurate account of the whole process. Fixed air is acknowledged to be a compound, of which one ingredient is found to constitute about $\frac{2}{7}$ of the whole atmospheric fluid; we mean vital air or the oxygen of Lavoisier. When this is combined with phlogiston, according to the doctrine of Stahl, or with charcoal, according to Lavoisier, the result is fixed air or carbonic acid. The change therefore which breathing makes on the air is the solution of this matter by vital air; and the use of air in breathing is the carrying off this noxious principle in the way of solution. When therefore the air is already so far saturated as not to dissolve this substance as fast as it is secreted, or must be secreted in the lungs, the animal suffers the pain of suffocation, or is otherwise mortally affected. Suffocation is not the only consequence; for we can remain for a number of seconds without breathing, and then we begin to feel the true pain of suffocation; but those who have been instantaneously struck down by an inspiration of fixed air, and afterwards recovered to life, complained of no such pain, and seemed to have suffered chiefly by a nervous affection. It is said (but we will not vouch for the truth of it), that a person may safely take a full inspiration of fixed air, if the passages of the nose be shut; and that unless these nerves be stimulated by the fixed air, it is not instantaneously mortal. But these are questions out of our present line of inquiry. They are questions of physiology, and are treated of in other works. Our business is to explain in what manner the pressure and elasticity of the air, combined with the structure and mechanism of the body, operate in producing this necessary secretion and removal of the matter discharged from the lungs in the act of breathing.

It is well ascertained, that the secretion is made from the mass of blood during its passage through the lungs. The blood delivered into the lungs is of a dark blackish colour, and it is there changed into a florid red. In the lungs it is exposed to the action of the air in a prodigiously extended surface: for the lungs consist of an inconceivable number of small vessels or bladders, communicating with each other and with the windpipe. These are filled with air in every inspiration. These vessels are everywhere in contact with minute blood-vessels. The blood does not *in toto* come into immediate contact with the air; and it would seem that it is only the thin serous part of it which is acted on by the air at the mouths of the vessels or pores, where it stands by capillary attraction. Dr Priestley found, that venous blood enclosed in thin bladders and other membranes was rendered florid by keeping the bladders in contact with abundance of pure vital air. We know also, that breath is moist or damp, and *must* have acquired this moisture in the lungs. It is immaterial whether this secretion of water or lymph (as the anatomists call it) be furnished by mere exudation through simple pores, or by a vascular and organic secretion; in either case, some ingredient of the blood comes in contact with air in the lungs, and there unites with it. This is farther confirmed, by observing, that all breathing animals are warmer than the surrounding medium, and that by every process in which fixed air is formed from vital air heat is produced. Hence this solution in air of something from the blood has been assigned by many as the source of animal heat. We touch on these things in a very transitory way in this place, only in order to prove that, for the support of animal life, there must be a very extensive application of air to the blood, and that this is made in the lungs.

The question before us in this place is, How is this brought about by the weight and elasticity of the air? This is done in two ways; by the action of the muscles of the ribs, and by the action of the diaphragm and other muscles of the abdomen. The thorax or chest is a great cavity, completely filled by the lungs. The sides of this cavity are formed by the ribs. These are crooked or arched, and each is moveable round its two ends, one of them being inserted into the vertebræ of the back, and the other into the sternum or breast-bone. The rib turns in a manner resembling the handle of a drawer. The inspection of Fig. 76 will illustrate this matter a little. Suppose the curves aie, bkf, clg, &c. to represent the ribs moveable round the extremities. Each succeeding rib is more bent than the one above it, and this curvature is both in the vertical and horizontal direction. Suppose each so broad as to project a little over its inferior like the tiles of a roof. It is evident, that if we take the lower one by its middle, and draw it out a little, moving it round the line np, it will bring out the next dmh along with it. Also, because the distance of the middle point o from the axis of motion np is greater than the distance of m from the axis dh, and because o will therefore describe a portion of a larger circle than m does, the rib nop will slide up a little under the rib dmh, or the rib dmh will overlap nop a little more than before; the distance om will therefore be diminished. The same must happen to all the superior ribs; but the change of distance will be less and less as we go upwards. Now, instead of this great breadth of the ribs overlapping each other, suppose each inferior rib connected with the one above it by threads or fibres susceptible of contraction at the will of man. The articulations a, e, of the first or upper rib with the spine and sternum are so broad and firm, that this rib can have little or no motion round the line ae; this rib therefore is as a fixture for the ends of all the contracting fibres: therefore, whenever the fibres which connect the second rib with the first rib contract, the second must rise a little, and also go outward, and will carry the lower ribs along with it; the third rib will rise still farther by the contraction of the muscles which connect it with the second, and so on: and thus the whole ribs are raised and thrown outward (and a little forward, because the articulation of each with the spine is considerably higher than that with the sternum), and the capacity of the thorax is enlarged by the contraction of its muscular covering. The direction of the muscular fibres is very oblique to the direction of the circular motion which it produces; from which circumstance it follows, that a very minute contraction of the muscles produces all the motion which is necessary. This indeed is not great; the whole motion of the lowest ribs is less than an inch in the most violent inspiration, and the whole contraction of the muscles of the 12 ribs does not exceed the eighth part of an inch, even supposing the intercostal muscles at right angles to the ribs; and being oblique, the contraction is still less. It would seem, that the intensity of the contractive power of a muscular fibre is easily obtained, but that the space through which it can be exerted is very limited; for in most cases nature places the muscles in situations of great mechanical disadvantage in this respect, in order to procure other conveniences.

But this is not the whole effect of the contraction of the intercostal muscles: since the compound action of the two sets of muscles, which cross each other from rib to rib like the letter X, is nearly at right angles to the rib, but is oblique to its plane, it tends to push the ribs closer to their articulations, and thus to press out the two pillars on which they are articulated. Thus, supposing *a f* (Fig. 77.) to represent the section of one of the vertebræ of the spine, and *c d* a section of the sternum, and *a b c*, *f e d*, two opposite ribs, with a lax thread *b e* connecting them. If this thread be pulled upwards by the middle *g* till it is tight, it will tend to pull the points *b* and *e* nearer to each other, and to press the vertebra *a f* and the sternum *c d* outwards. The spine being the chief pillar of the body, may be considered as immoveable in the present instance. The sternum is sufficiently susceptible of motion for the present purpose. It remains almost fixed atop at its articulation with the first rib, but it gradually yields below; and thus the capacity of the thorax is enlarged in this direction also. The whole enlargement of the diameters of the thorax during inspiration is very small, not exceeding the fiftieth part of an inch in ordinary cases. This is easily calculated. Its quiescent capacity is about two cubic feet, and we never draw in more than 15 inches. Two spheres, one of which holds 2 cubic feet and the other 2 feet and 15 inches, will not differ in diameter above the fiftieth part of an inch.

The other method of enlarging the capacity of the thorax is very different. It is separated from the abdomen by a strong muscular partition called the *diaphragm*, which is attached to firm parts all around. In its quiescent or relaxed state it is considerably convex upwards, that is, towards the thorax, rising up into its cavity like the bottom of an ordinary quart bottle, only not so regular in its shape. Many of its fibres tend from its middle to the circumference, where they are inserted into firm parts of the body. Now suppose these fibres to contract. This must draw down its middle, or make it flatter than before, and thus enlarge the capacity of the thorax.

Physiologists are not well agreed as to the share which each of these actions has in the operation of enlarging the thorax. Many refuse all share of it to the intercostal muscles, and say that it is performed by the diaphragm alone. But the fact is, that the ribs are really observed to rise even while the person is asleep; and this cannot possibly be produced by the diaphragm, as these anatomists assert. Such an opinion shows either ignorance or neglect of the laws of pneumatics. If the capacity of the thorax were enlarged only by drawing down the diaphragm, the pressure of the air would compress the ribs, and make them descend. And the simple laws of mechanics make it as evident as any proposition in geometry, that the contraction of the intercostal muscles *must* produce an elevation of the ribs and enlargement of the thorax; and it is one of the most beautiful contrivances of nature. It depends much on the will of the animal what share each of these actions shall have. In general, the greatest part is done by the diaphragm; and any person can breathe in such a manner that his ribs shall remain motionless; and, on the contrary, he can breathe almost entirely by raising his chest. In the first method of breathing, the belly rises during inspiration, because the contraction of the diaphragm compresses the upper part of the bowels, and therefore squeezes them outwards; so that an ignorant person would be apt to think that the breathing was performed by the belly, and that the belly is inflated with the air. The strait lacing of the women impedes the motion of the ribs, and changes the natural habit of breathing, or brings on an unnatural habit. When the mind is depressed, it is observed that the breathing is more performed by the muscles of the thorax; and a deep sigh is always made in this way.

These observations on the manner in which the capacity of the chest can be enlarged were necessary, before we can acquire a just notion of the way in which the mechanical properties of air operate in applying it to the mass of blood during its passage through the lungs. Suppose the thorax quite empty, and communicating with the external air by means of the trachea or windpipe, it would then resemble a pair of bellows. Raising the boards corresponds to the raising of the ribs; and we might imitate the action of the diaphragm by forcibly pulling outwards the folded leather which unites them. Thus their capacity is enlarged, and the air rushes in at the nozzle by its weight in the same manner as water would do. The thorax differs from bellows only in this respect, that it is filled by the lungs, which is a vast collection of little bladders, like the holes in a piece of fermented bread, all communicating with the trachea, and many of them with each other. When the chest is enlarged, the air rushes into them all in the same manner as into the single cavity of an empty thorax. It cannot be said with propriety that they are inflated: all that is done is the *allowing* the air to come in. At the same time, as their membranous covering must have some thickness, however small, and some elasticity, it is not unlikely that, when compressed by expiration, they tend a little to recover their former shape, and thus aid the voluntary action of the muscles. It is in this manner that a small bladder of caoutchouc swells again after compression, and fills itself with air or water. But this cannot happen except in the most minute vesicles: those of sensible bulk have not elasticity enough for this purpose. The lungs of birds, however, have some very large bladders, which have a very considerable elasticity, and recover their shape and size with great force after compression, and thus fill themselves with air. The respiration of these animals is considerably different from that of land animals, and their muscles act chiefly in expiration. This will be explained by and by as a curious variety in the pneumatic instrument.

This account of the manner in which the lungs are filled with air does not seem agreeable to the notions we entertain of it. We seem to suck in the air; but although it be true that we act, and exert force, in order to get air into our lungs, it is not by our action, but by external pressure, that it does come in. If we apply our mouth to the top of a bottle filled with water, we find that no draught, as we call it, of our chest will suck in any of the water; but if we suck in the very same manner at the end of a pipe immersed in water, it follows immediately. Our interest in the thing makes us connect in imagination our own action with the effect, without thinking on the many steps which may intervene in the train of natural operations; and we consider the action as the immediate cause of the air's reception into the lungs. It is as if we opened the door, and took in by the hand a person who was really pushed in by the crowd without. If an incision be made into the side of the thorax, so that the air can get in by that way, when the animal acts in the usual manner, the air will really come in by this hole, and fill the space between the lungs and thorax; but no air is sucked into the lungs by this process, and the animal is as completely suffocated as if the windpipe were shut up. And, on the other hand, if a hole be made into the lungs without communicating with the thorax, the animal will breathe through this hole, though the windpipe be stopped. This is successfully performed in cases of patients whose trachea is shut up by accident or by inflammation; only it is necessary that this perforation be made into a part of the lungs where it may meet with some of the great pulmonary passages; for if made into some remote part of a lobe, the air cannot find its way into the rest of the lungs through such narrow passages, obstructed too by blood, &c.

We have now explained, on pneumatical principles, the process of inspiration. The expiration is chiefly performed by the natural tone of the parts. In the act of inspiration the ribs were raised and drawn outwards in opposition to the elasticity of the solids themselves; for although the ribs are articulated at their extremities, the articulations are by no means such as to give a free and easy motion like the joints of the limbs. This is particularly the case in the articulations with the sternum, which are by no means fitted for motion. It would seem that the motion really produced here is chiefly by the yielding of the cartilaginous parts and the bending of the rib; when therefore the muscles which produced this effect are allowed to relax, the ribs again collapse. Perhaps this is assisted a little by the action of the long muscles which come down across the ribs without being inserted into them. These may draw them together a little, as we compress a loose bundle by a string.

In like manner, when the diaphragm was drawn down, it compressed the abdomen in opposition to the elasticity of all the viscera contained in it, and to the elasticity and tone of the teguments and muscles which surround it. When therefore the diaphragm is relaxed, these parts push it up again into its natural situation, and in doing this expel the air from the lungs.

If this be a just account of the matter, expiration should be performed without any effort. This accordingly is the case. We feel that, after having made an ordinary easy inspiration, it requires the continuance of the effort to keep the thorax in this enlarged state, and that all that is necessary for expiration is to cease to act. No person feels any difficulty in emptying the lungs; but weak people often feel a difficulty of inspiration, and compare it to the feeling of a weight on their breast; and expiration is the last motion of the thorax in a dying person.

But nature has also given us a mechanism by which we can expire, namely, the abdominal muscles; and when we have finished an ordinary and easy expiration, we can still expel a considerable bulk of air (nearly half of the contents of the lungs) by contracting the abdominal muscles. These, by compressing the body, force up its moveable contents against the diaphragm, and cause it to rise further into the thorax, acting in the same manner as when we expel the fæces *per anum*. When a person breathes out as much air as he can in this manner, he may observe that his ribs do not collapse during the whole operation.

There seems then to be a certain natural unconstrained state of the vesicles of the lungs, and a certain quantity of air necessary for keeping them of this size. It is probable that this state of the lungs gives the freest motion to the blood. Were they more compressed, the blood-vessels would be compressed by the adjoining vesicles; were they more lax, the vessels would be more crooked, and by this means obstructed. The frequent inspirations gradually change this air by mixing fresh air with it, and at every expiration carrying off some of it. In catarrhs and inflammations, especially when attended with suppuration, the small passages into the remote vessels are obstructed, and thus the renewal of air in them will be prevented. The painful feeling which this occasions causes us to expel the air with violence, shutting the windpipe, till we have exerted strongly with the abdominal muscles, and made a strong compression on the lower part of the thorax. We then open the passage suddenly, and expel the air and obstructing matter by violent coughing.

We have said, that birds exhibit a curious variety in the process of breathing. The muscles of their wings being so very great, required a very extensive insertion, and this is one use of the great breast-bone. Another use of it is, to form a firm partition to hinder the action of these muscles from compressing the thorax in the act of flying: therefore the form of their chest does not admit of alternate enlargement and contraction to that degree as in land animals. Moreover, the muscles of their abdomen are also very small; and it would seem that they are not sufficient for producing the compression on the bowels which is necessary for carrying on the process of concoction and digestion. Instead of aiding the lungs, they receive help from them.

In an ostrich, the lungs consist of a fleshy part A, A (Fig. 78.), composed of vesicles like those of land animals, and, like theirs, serving to expose the blood to the action of the air. Besides these, they have on each side four large bags B, C, D, E, each of which has an orifice G communicating with the trachea; but the second, C, has also an orifice H, by which it communicates with another bag F situated below the rest in the abdomen. Now, when the lungs are compressed by the action of the diaphragm, the air in C is partly expelled by the trachea through the orifice G, and partly driven through the orifice H into the bag F, which is then allowed to receive it; because the same action which compresses the lungs enlarges the abdomen. When the thorax is enlarged, the bag C is partly supplied with fresh air through the trachea, and partly from the bag F. As the lungs of other animals resemble a common bellows, the lungs of birds resemble the smith's bellows with a partition; and anatomists have discovered passages from this part of the lungs into their hollow bones and quills. We do not know all the uses of this contrivance; and only can observe, that this alternate action must assist the muscles of the abdomen in promoting the motion of the food along the alimentary canal, &c. We can distinctly observe in birds that their belly dilates when the chest collapses, and *vice versa*, contrary to what we see in the land animals. Another use of this double passage may be to produce a circulation of air in the lungs, by which a compensation is made for the smaller surface of action on the blood: for the number of small vesicles, of equal capacity with these large bags, gives a much more extensive surface.

If we try to raise mercury in a pipe by the action of the chest alone, we cannot raise it above two or three inches; and the attempt is both painful and hazardous. It is painful chiefly in the breast, and it provokes coughing. Probably the fluids ooze through the pores of the vesicles by the pressure of the surrounding parts.

On the other hand, we can by expiration support mercury about five or six inches high: but this also is very painful, and apt to produce extravasation of blood. This seems to be done entirely by the abdominal muscles.

The operation properly termed SUCKING is totally different from breathing, and resembles exceedingly the action of a common pump. Suppose a pipe held in the mouth, and its lower end immersed in water. We fill the mouth with the tongue, bringing it forward, and applying it closely to the teeth and to the palate; we then draw it back, or bend it downwards (behind) from the palate, thus leaving a void. The pressure of the air on the cheeks immediately depresses them, and applies them close to the gums and teeth; and its pressure on the water in the vessel causes it to rise through the pipe into the empty part of the mouth, which it quickly fills. We then push forward the tip of the tongue, below the water, to the teeth, and apply it to them all round, the water being above the tongue, which is kept much depressed. We then apply the tongue to the palate, beginning at the tip, and gradually going backward in this application. By this means the water is gradually forced backward by an operation similar to that of the gullet in swallowing. This is done by contracting the gullet above, and relaxing it below, just as we would empty a gut of its contents by drawing our closed hand along it. By this operation the mouth is again completely occupied by the tongue, and we are ready for repeating the operation. Thus the mouth and tongue resemble the barrel and piston of a pump; and the application of the tip of the tongue to the teeth performs the office of the valve at the bottom of the barrel, preventing the return of the water into the pipe.

Although usual, it is not absolutely necessary to withdraw the tip of the tongue, making a void before the tongue. Sucking may be performed by merely separating the tongue gradually from the palate, beginning at the root. If we withdraw the tip of the tongue a very minute quantity, the water gets in and flows back above the tongue.

The action of the tongue in this operation is very powerful; some persons can raise mercury 25 inches: but this strong exertion is very fatiguing, and the soft parts are prodigiously swelled by it. It causes the blood to ooze plentifully through the pores of the tongue, fauces, and palate, in the same manner as if a cupping-glass and syringe were applied to them; and, when the inside of the mouth is excoriated or tender, as is frequent with infants, even a very moderate exertion of this kind is accompanied with extravasation of blood. When children suck the nurse's breast, the milk follows their exertion by the pressure of the air on the breast; and a weak child, or one that withholds its exertions on account of pain from the above-mentioned cause, may be assisted by a gentle pressure of the hand on the breast: the infant pupil of nature, without any knowledge of pneumatics, frequently helps itself by pressing its face to the yielding breast.

In the whole of this operation the breathing is performed through the nostrils; and it is a prodigious distress to an infant when this passage is obstructed by mucous. We beg to be forgiven for observing by the way, that this obstruction may be almost certainly removed for a little while, by rubbing the child's nose with any liquid of quick evaporation, or even with water.

The operation in drinking is not very different from that in sucking: we have indeed little occasion here to suck, but we must do it a little. Dogs and some other animals cannot drink, but only lap the water into their mouths with their tongue, and then swallow it. The gallinaceous birds seem to drink very imperfectly; they seem merely to dip their head into the water up to the eyes till their mouth is filled with water, and then holding up the head, it gets into the gullet by its weight, and is then swallowed. The elephant drinks in a very complicated manner; he dips his trunk into the water, and fills it by making a void in his mouth: this he does in the contrary way to man. After having depressed his tongue, he begins the application of it to the palate at the root, and by extending the application forward, he expels the air by the mouth which came into it from the trunk. The process here is not very unlike that of the condensing syringe without a piston valve, formerly described, in which the external air (corresponding here to the air in the trunk) enters by the hole F in the side, and is expelled through the hole in the end of the barrel; by this operation the trunk is filled with water: then he lifts his trunk out of the water, and bringing it to his mouth, pours the contents into it, and swallows it. On considering this operation, it appears that, by the same process by which the air of the trunk is taken into the mouth, the water could also be taken in, to be afterwards swallowed: but we do not find, upon inquiry, that this is done by the elephant; we have always observed him to drink in the manner now described. In either way it is a double operation, and cannot be carried on any way but by alternately sucking and swallowing, and while one operation is going on the other is interrupted; whereas man can do both at the same time.

There is an operation similar to that of the elephant, which many find a great difficulty in acquiring, viz. keeping up a continued blast with a blow-pipe. We would desire our chemical reader to attend minutely to the gradual action of his tongue in sucking, and he will find it such as we have described. Let him attend particularly to the way in which the tip of the tongue performs the office of a valve, preventing the return of the water into the pipe; the same position of the tongue would hinder air from coming into the mouth. Next let him observe, that in swallowing what water he has now got lodged above his tongue, he continues the tip of the tongue applied to the teeth; now let him shut his mouth, keeping his lips firm together, the tip of the tongue at the teeth, and the whole tongue forcibly kept at a distance from the palate; bring up the tongue to the palate, and allow the tip to separate a little from the teeth; this will expel the air into the space between the fauces and cheeks, and will blow up the cheeks a little: then, acting with the tip of the tongue as a valve, hinder this air from getting back, and depressing the tongue again, more air (from the nostrils) will get into the mouth, which may be expelled into the space without the teeth as before, and the cheeks will be more inflated: continue this operation, and the lips will no longer be able to retain it, and it will ooze through as long as the operation is continued. When this has become familiar and easy, take the blow-pipe, and there will be no difficulty in maintaining a blast as uniform as a smith's bellows, breathing all the while through the nostrils. The only difficulty is the holding the pipe: this fatigues the lips; but it may be removed by giving the pipe a convenient shape, a pretty flat oval, and wrapping it round with leather or thread.

Another phenomenon depending on the principles already established, is the land and sea-breeze in the warm countries.

We have seen that air expands exceedingly by heat; therefore heated air, being lighter than an equal bulk of cold air, must rise in it. If we lay a hot stone in the sunshine in a room, we shall observe the shadow of the stone surrounded with a fluttering shadow of different degrees of brightness, and that this flutter rises rapidly in a column above the stone. If we hold an extinguished candle near the stone, we shall see the smoke move towards the stone, and then ascend up from it. Now, suppose an island receiving the first rays of the sun in a perfectly calm morning; the ground will soon be warmed, and will warm the contiguous air. If the island be mountainous, this effect will be more remarkable; because the inclined sides of the hills will receive the light more directly: the midland air will therefore be most warmed: the heated air will rise, and that in the middle will rise fastest: and thus a current of air upwards will begin, which must be supplied by air coming in from all sides, to be heated and to rise in its turn; and thus the morning *sea-breeze* is produced, and continues all day. This current will frequently be reversed during the night, by the air cooling and gliding down the sides of the hills, and we shall have the *land-breeze*.

It is owing to the same cause that we have a circulation of air in mines which have the mouths of their shafts of unequal heights. The temperature underground is pretty constant through the whole year, while that of the atmosphere is extremely variable. Now, suppose a mine having a long horizontal drift, communicating between two pits or shafts, and that one of these shafts terminates in a valley, while the other opens on the brow of a hill perhaps 100 feet higher. Let us further suppose it summer, and the air heated to 65°, while the temperature of the earth is but 45°; this last will be also the temperature of the air in the shafts and the drift. Now, since air expands nearly 24 parts in 10,000 by one degree of heat, we shall have an odds of pressure at the bottom of the two shafts equal to nearly the 20th part of the weight of a column of air 100 feet high (100 feet being supposed the difference of the heights of the shafts). This will be about six ounces on every square foot of the section of the shaft. If this pressure could be continued, it would produce a prodigious current of air down the long shaft, along the drift, and up the short shaft. The weight of the air acting through 100 feet would communicate to it the velocity of 80 feet *per* second: divide this by $\sqrt{20}$, that is, by 4,5, and we shall have 18 feet *per* second for the velocity: this is the velocity of what is called a brisk gale. This pressure *would* be continued, if the warm air which enters the long shaft were cooled and condensed as fast as it comes in; but this is not the case. *It is however cooled* and condensed, and a current is produced sufficient to make an abundant circulation of air along the whole passage; and care is taken to dispose the shafts and conduct the passages in such a manner that no part of the mine is out of the circle. When any new lateral drift is made, the renewal of air at its extremity becomes more imperfect as it advances; and when it is carried a certain length, the air stagnates and becomes suffocating, till either a communication can be made with the rest of the mine, or a shaft be made at the end of this drift.

As this current depends entirely on the difference of temperature between the air below and that above, it must cease when this difference ceases. Accordingly, in the spring and autumn, the miners complain much of stagnation; but in summer they never want a current from the deep pits to the shallow, nor in the winter a current from the shallow pits to the deep ones. It frequently happens also, that in mineral countries the chemical changes which are going on in different parts of the earth make differences of temperature sufficient to produce a sensible current.

It is easy to see that the same causes must produce a current down our chimneys in summer. The chimney is colder than the summer air, and must therefore condense it, and it will come down and run out at the doors and windows.

And this naturally leads us to consider a very important effect of the expansion and consequent ascent of air by heat, namely, the drawing (as it is called) of chimneys. The air which has contributed to the burning of fuel must be intensely heated, and will rise in the atmosphere. This will also be the case with much of the surrounding air which has come very near the fire, although not in contact with it. If this heated air be made to rise in a pipe, it will be kept together, and therefore will not soon cool and collapse: thus we shall obtain a long column of light air, which will rise with a force so much the greater as the column is longer or more heated. Therefore the taller we make the chimney, or the hotter we make the fire, the more rapid will be the current, or the draught or suction, as it is injudiciously called, will be so much the greater. The ascensional force is the difference between the weight of the column of heated air in the funnel and a column of the surrounding atmosphere of equal height. We increase the draught, therefore, by increasing the perpendicular height of the chimney. Its length in a horizontal direction gives no increase, but, on the contrary, diminishes the draught, by cooling the air before it gets into the effective part of the funnel. We increase the draught also by obliging all the air which enters the chimney to come very near the fuel; therefore a low mantle-piece will produce this effect; also filling up all the spaces on each side of the grate. When much air gets in above the fire, by having a lofty mantle-piece, the general mass of air in the chimney cannot be much heated. Hence it must happen that the greatest draught will be produced by bringing down the mantle-piece to the very fuel; but this converts a fire-place into a furnace, and by thus sending the whole air through the fuel, causes it to burn with great rapidity, producing a prodigious heat; and this producing an increase of ascensional force, the current becomes furiously rapid, and the heat and consumption of fuel immense. If the fire-place be a cube of a foot and a half, and the front closed by a door, so that all the air must enter through the bottom of the grate, a chimney of 15 or 20 feet high, and sufficiently wide to give passage to all the expanded air which can pass through the fire, will produce a current which will roar like thunder, and a heat sufficient to run the whole inside into a lump of glass.

All that is necessary, however, in a chamber fire-place, is a current sufficiently great for carrying up the smoke and vitiated air of the fuel. And as we want also the enlivening flutter and light of the fire, we give the chimney-piece both a much greater height and width than what is merely necessary for carrying up the smoke, only wishing to have the current sufficiently determinate and steady for counteracting any occasional tendency which it may sometimes have to come into the room. By allowing a greater quantity of air to get into the chimney, heated only to a moderate degree, we produce a more rapid renewal of the air of the room: did we oblige it to come so much nearer the fire as to produce the same renewal of the air in consequence of a more rapid current, we should produce an inconvenient heat. But in this country, where pit-coal is in general so very cheap, we carry this indulgence to an extreme; or rather, we have not studied how to get all the desired advantages with economy. A much smaller renewal of air than we commonly produce is abundantly wholesome and pleasant, and we may have all the pleasure of the light and flame of the fuel at much less expense, by contracting greatly the passage into the vent. The best way of doing this is by contracting the brick-work on each side behind the mantle-piece, and reducing it to a narrow parallelogram, having the back of the vent for one of its long sides. Make an iron plate to fit this hole, of the same length, but broader, so that it may lie sloping, its lower edge being in contact with the foreside of the hole, and its upper edge leaning on the back of the vent. In this position it shuts the hole entirely. Now let the plate have a hinge along the front or lower edge, and fold up like the lid of a chest. We shall thus be able to enlarge the passage at pleasure. In a fire-place fit for a room of 24 feet by 18, if this plate may be about 18 inches long from side to side, and folded back within an inch or an inch and a half of the wall, this will allow passage for as much air as will keep up a very cheerful fire; and by raising or lowering this REGISTER, the fire may be made to burn more or less rapidly. A free passage of half an inch will be sufficient in weather that is not immoderately cold. The principle on which this construction produces its effect is, that the air which is in the front of the fire, and much warmed by it, is not allowed to get into the chimney, where it would be immediately hurried up the vent, but rises up to the ceiling, and is diffused over the whole room. This double motion of the air may be distinctly observed by opening a little of the door and holding a candle in the way. If the candle be held near the floor, the flame will be blown into the room; but if held near the top of the door, the flame will be blown outward.

But the most perfect method of warming an apartment in these temperate climates, where we can indulge in the cheerfulness and sweet air produced by an open fire, is what we call a stove-grate, and our neighbours on the continent call a chapelle, from its resemblance to the chapels or oratories in the great churches.

In the great chimney-piece, which, in this case, may be made even larger than ordinary, is set a smaller one fitted up in the same style of ornament, but of a size no greater than is sufficient for holding the fuel. The sides and back of it are made of iron (cast iron is preferable to hammered iron, because it does not so readily calcine), and are kept at a small distance from the sides and back of the main chimney-piece, and are continued down to the hearth, so that the ash-pit is also separated. The pipe or chimney of the stove grate is carried up behind the ornaments of the mantle-piece till it rises above the mantle-piece of the main chimney-piece, and is fitted with a register or damper-plate turning round a transverse axis. The best form of this register is that which we have recommended for an ordinary fire-place, having its axis or joint close at the front; so that when it is open or turned up, the burnt air and smoke striking it obliquely, are directed with certainty into the vent, without any risk of reverberating and coming out into the room. All the rest of the vent is shut up by iron plates or brick-work out of sight.

The effect of this construction is very obvious. The fuel, being in immediate contact with the back and sides of the grate, heat them to a great degree, and they heat the air contiguous to them. This heated air cannot get up the vent, because the passages above these spaces are shut up. It therefore comes out into the room; some of it goes into the real fire-place and is carried up the vent, and the rest rises to the ceiling, and is diffused over the room.

It is surprising to a person who does not consider it with skill how powerfully this grate warms a room. Less than one-fourth of the fuel consumed in an ordinary fire-place is sufficient; and this with the same cheerful blazing hearth and salutary renewal of air. It even requires attention to keep the room cool enough. The heat communicated to those parts in contact with the fuel is needlessly great; and it will be a considerable improvement to line this part with very thick plates of cast iron, or with tiles made of fire-clay, which will not crack with the heat. These, being very bad conductors, will make the heat, ultimately communicated to the air, very moderate. If, with all these precautions, the heat should be found too great, it may be brought under perfect management by opening passages into the vent from the lateral spaces. These may be valves or trap-doors moved by rods concealed behind the ornaments.

Thus we have a fire-place under the most complete regulation, where we can always have a cheerful fire without being for a quarter of an hour incommoded by the heat; and we can as quickly raise our fire, when too low, by hanging on a plate of iron on the front, which shall reach as low as the grate. This in five minutes will blow up the fire into a glow; and the plate may be sent out of the room, or set behind the stove-grate out of sight.

The propriety of enclosing the ash-pit is not so obvious; but if this be not done, the light ashes, not finding a ready passage up the chimney, will come out into the room along with the heated air.

We do not consider in this place the various extraneous circumstances which impede the current of air in our chimneys, and produce smoky houses; but only the theory of this motion in general, and the modifications of its operation arising from the various purposes to which it may be applied.

Under this head we shall next give a general account and description of the method of warming apartments by stoves. A STOVE in general is a fire-place shut up on all sides, having only a passage for admitting the air to support the fire, and a tube for carrying off the vitiated air and smoke; and the air of the room is warmed by coming into contact with the outside of the stove and flue. The general principle of construction, therefore, is very simple. The air must be made to come into as close contact as possible with the fire, or even to pass through it, and this in such quantities as just to consume a quantity of fuel sufficient for producing the heat required; and the stove must be so constructed, that both the burning fuel and the air which has been heated by it shall be applied to as extensive a surface as possible of furnace, all in contact with the air of the room; and the heated air within the stove must not be allowed to get into the funnel which is to carry it off, till it is too much cooled to produce any considerable heat on the outside of the stove.

In this temperate climate no great ingenuity is necessary for warming an ordinary apartment; and stoves are made rather to please the eye as furniture than as economical substitutes for an open fire of equal calorific power. But our neighbours on the continent, and especially towards the north, where the cold of winter is intense, and fuel very dear, have bestowed much attention on their construction, and have combined ingenious economy with every elegance of form. Nothing can be handsomer than the stoves of Fayencerie that are to be seen in French Flanders, or the Russian stoves at St Petersburgh, finished in stucco. Our readers will not, therefore, be displeased with a description of them. In this place, however, we shall only consider a stove in general as a subject of pneumatical discussion.

The general form, therefore, of a stove, and of which all others are only modifications adapted to circumstances of utility or taste, is as follows:

MIKL (Fig. 79.) is a quadrangular box of any size in the directions MI LK. The inside width from front to back is pretty constant, never less than ten inches, and rarely extending to twenty; the included space is divided by a great many partitions. The lowest chamber AB is the receptacle for the fuel, which lies on the bottom of the stove without any grate: this fire-place has a door AO turning on hinges, and in this door is a very small wicket P: the roof of the fire-place extends to within a very few inches of the farther end, leaving a narrow passage B for the flame. The next partition *b* B is about eight inches higher, and reaches almost to the other end, leaving a narrow passage for the flame at B. The partitions are repeated above, at the distance of eight inches, leaving passages at the ends, alternately disposed as in the figure; the last of them H communicates with the room vent. This communication may be regulated by a plate of iron, which can be slid across it by means of a rod or handle which comes through the side. The more usual way of shutting up this passage is by a sort of pan or bowl of earthen ware which is whelmed over it with its brim resting in sand contained in a groove formed all round the hole. This damper is introduced by a door in the front, which is then shut. The whole is set on low pillars, so that its bottom may be a few inches from the floor of the room: it is usually placed in a corner, and the apartments are so disposed that their chimneys can be joined in stacks as with us.

Some straw or wood-shavings are first burnt on the hearth at its farther end. This warms the air in the stove, and creates a determined current. The fuel is then laid on the hearth close by the door, and pretty much piled up. It is now kindled; and the current being already directed to the vent, there is no danger of any smoke coming out into the room. Effectually to prevent this, the door is shut, and the wicket P opened. The air supplied by this, being directed to the middle or bottom of the fuel, quickly kindles it, and the operation goes on.

The aim of this construction is very obvious. The flame and heated air are retained as long as possible within the body of the stove by means of the long passages; and the narrowness of these passages obliges the flame to come in contact with every particle of soot, so as to consume it completely, and thus convert the whole combustible matter of the fuel into heat. For want of this a very considerable portion of our fuel is wasted by our open fires, even under the very best management: the soot which sticks to our vents is very inflammable, and a pound weight of it will give as much if not more heat than a pound of coal. And what sticks to our vents is very inconsiderable in comparison with what escapes unconsumed at the chimney top. In fires of green wood, peat, and some kinds of pit-coal, nearly one-fifth of the fuel is lost in this way; but in these stoves there is hardly ever any mark of soot to be seen; and even this small quantity is produced only after lighting the fires. The volatile inflammable matters are expelled from parts much heated indeed, but not so hot as to burn; and some of it charred or half-burnt cannot be any further consumed, being enveloped in flame and air already vitiated and unfit for combustion. But when the stove is well heated, and the current brisk, no part of the soot escapes the action of the air.

The hot air retained in this manner in the body of the stove is applied to its sides in a very extended surface. To increase this still more, the stove is made narrower from front to back in its upper part; a certain breadth is necessary below, that there may be room for fuel. If this breadth were preserved all the way up, much heat would be lost, because the heat communicated to the partitions of the stove does no good. By diminishing their breadth, the proportion of useful surface is increased. The whole body of the stove may be considered as a long pipe folded up, and its effect would be the greatest possible if it really were so; that is, if each partition *c* C, *d* D, &c. were split into two, and a free passage allowed between them for the air of the room. Something like this will be observed afterwards in some German stoves.

It is with the same view of making an extensive application of a hot surface to the air, that the stove is not built on the wall, nor even in contact with it, nor with the floor:

for by its detached situation, the air in contact with the back, and with the bottom (where it is hottest), is warmed, and contributes at least one-half of the whole effect; for the great heat of the bottom makes its effect on the air of the room at least equal to that of the two ends. Sometimes a stove makes part of the wall between too small rooms, and is found sufficient.

It must be remarked, on the whole, that the effect of a stove depends much on keeping in the room the air already heated by it. This is so remarkably the case, that a small open fire in the same room will be so far from increasing its heat, that it will greatly diminish it: it will even draw the warm air from a suite of adjoining apartments. This is distinctly observed in the houses of the English merchants in St Petersburgh: their habits of life in Britain make them uneasy without an open fire in their sitting rooms; and this obliges them to heat all their stoves twice a-day, and their houses are cooler than those of the Russians who heat them only once. In many German houses, especially of the lower class, the fire-place of the stove does not open into the room, but into the yard or lobby, where all the fires are lighted and tended; by this means is avoided the expense of warm air which must have been carried off by the stove: but it is evident, that this must be very unpleasant, and cannot be wholesome. We must breathe the same quantity of stagnant air, loaded with all the vapours and exhalations which must be produced in every inhabited place. Going into one of these houses from the open air, is like putting one's head into a stew-pan or under a pie-crust, and quickly nauseates us who are accustomed to fresh air and cleanliness. In these countries it is a matter almost of necessity to fumigate the rooms with frankincense and other gums burnt. The censer in ancient worship was in all probability an utensil introduced by necessity for sweetening or rendering tolerable the air of a crowded place:

and it is a constant practice in the Russian houses for a servant to go round the room after dinner, waving a censer with some gums burning on bits of charcoal.

The account now given of stoves for heating rooms, and of the circumstances which must be attended to in their construction, will equally apply to hot walls in gardening, whether within or without doors. The only new circumstance which this employment of a flue introduces, is the attention which must be paid to the equability of the heat, and the gradation which must be observed in different parts of the building. The heat in the flue gradually diminishes as it recedes from the fire-place, because it is continually giving out heat to the flue. It must therefore be so conducted through the building by frequent returns, that in every part there may be a mixture of warmer and cooler branches of the flue, and the final chimney should be close by the fire-place. It would, however, be improper to run the flue from the end of the floor up to the ceiling, where the second horizontal pipe would be placed, and then return it downward again, and make the third horizontal flue adjoining to the first, &c. This would make the middle of the wall the coldest. If it is the flue of a greenhouse, this would be highly improper, because the upper part of the wall can be very little employed; and in this case it is better to allow the flue to proceed gradually up the wall in its different returns, by which the lowest part would be the warmest, and the heated air will ascend among the pots and plants; but in a hot wall, where the trees are to receive heat by contact, some approximation to the above method may be useful.

In the hypocausta and sudaria of the Greeks and Romans, the flue was conducted chiefly under the floors.

Malt-kilns are a species of stove which merit our attention. Many attempts have been made to improve them on the principle of flue-stoves; but they have been unsuccessful, because heat is not what is chiefly wanted in malting: it is a copious current of very dry air to carry off the moisture.

All that is to be attended to in the different kinds of melting furnaces is, that the current of air be sufficiently rapid, and that it be applied in as extensive a surface as possible to the substance to be melted. The more rapid the current it is the hotter, because it is consuming more fuel; and therefore its effect increases in a higher proportion than its rapidity. It is doubly effectual if twice as hot; and if it then be twice as rapid, there is twice the quantity of doubly hot air applied to the subject; it would therefore be four times more powerful. This is procured by raising the chimney of the furnace to a greater height. The close application of it to the subject can hardly be laid down in general terms, because it depends on the precise circumstances of each case.

In reverberatory furnaces, such as refining furnaces for gold, silver, and copper, the flame is made to play over the surface of the melted metal. This is produced entirely by the form of the furnace, by making the arch of the furnace as low as the circumstances of the manipulation will allow. Experience has pointed out in general the chief circumstances of their construction, viz. that the fuel should be at one end on a grate, through which the air enters to maintain the fire, and that the metal should be placed on a level floor between the fuel and the tall chimney which produces the current. But there is no kind of furnace more variable in its effect, and almost every place has a small peculiarity of construction, on which its pre-eminence is rested. This has occasioned many whimsical varieties in their form. This uncertainty seems to depend much on a circumstance rather foreign to our present purpose; but as we do not observe it taken notice of by mineralogical writers, we beg leave to mention it here. It is not heat alone that is wanted in the refining of silver by lead, for instance. We must make a continual application to its surface of air, which has not contributed to the combustion of the fuel. Any quantity of the hottest air, already saturated with the fuel, may play on the surface of the metal for ever, and keep it in the state of most perfect fusion, but without refining it in the least. Now, in the ordinary construction of a furnace, this is much the case. If the whole air has come in by the grate, and passed through the middle of the fuel, it can hardly be otherwise than nearly saturated with it; and if air be also admitted by the door (which is generally done or something equivalent), the pure air lies above the vitiated air, and during the passage along the horizontal part of the furnace, and along the surface of the metal, it still keeps above it, at least there is nothing to promote their mixture. Thus the metal does not come into contact with air fit to act on the base metal and calcine it, and the operation of refining goes on slowly. Trifling circumstances in the form of the arch or canal may tend to promote the jumbling of the airs together, and thus render the operation more expeditious; and as these are but ill understood, or perhaps this circumstance not attended to, no wonder that we see these considered as so many nostrums of great importance. It were therefore worth while to try the effect of changes in the form of the roof directed to this very circumstance. Perhaps some little prominence down from the arch of the reverberatory would have this effect, by suddenly throwing the current into confusion. If the additional length of passage do not cool the air too much, we should think that if there were interposed between the fuel and the refining floor a passage twisted like a cork-screw, making just half a turn, it would be most effectual: for we imagine, that the two airs, keeping each to their respective sides of the passage, would by this means be turned upside down, and that the pure stratum would now be in contact with the metal, and the vitiated air would be above it.

The glass-house furnace exhibits the chief variety in the management of the current of heated air. In this it is necessary that the hole at which the workman dips his pipe into the pot shall be as hot as any part of the furnace. This could never be the case, if the furnace had a chimney situated in a part above the dipping-hole; for in this case cold air would immediately rush in at the hole, play over the surface of the pot, and go up the chimney. To prevent this the hole itself is made the chimney; but as this would be too short, and would produce very little current and very little heat, the whole furnace is set under a tall dome. Thus the heated air from the real furnace is confined in this dome, and constitutes a high column of very light air, which will therefore rise with great force up the dome, and escape at the top. This dome is therefore the chimney, and will produce a draught or current proportioned to its height. Some are raised above an hundred feet. When all the doors of this house are shut, and thus no supply given except through the fire, the current and heat become prodigious. This, however, cannot be done, because the workmen are in this chimney, and must have respirable air. But notwithstanding this supply by the house-doors, the draught of the real furnace is vastly increased by the dome, and a heat produced sufficient for the work, and which could not have been produced without the dome.

This has been applied with great ingenuity and effect to a furnace for melting iron from the ore, and an iron finery, both without a blast. The common blast iron furnace is well known. It is a tall cone with the apex undermost. The ore and fluxes are thrown into this cone mixed intimately with the fuel till it is full, and the blast of most powerful bellows is directed into the bottom of this cone through a hole in the side. The air is thrown in with such force, that it makes its way through the mass of matter, kindles the fuel in its passage, and fluxes the materials, which then drop down into a receptacle below the blast-hole, and thus the passage for the air is kept unobstructed. It was thought impossible to produce or maintain this current without bellows; but Mr Cotterel, an ingenious founder, tried the effect of a tall dome placed over the mouth of the furnace, and though it was not half the height of many glass-house domes it had the desired effect. Considerable difficulties, however, occurred; and he had not surmounted them all when he left the neighbourhood of Edinburgh, nor have we heard that he has yet brought the invention to perfection. It is extremely difficult to place the holes below, at which the air is to enter, at such a precise height as neither to be choked by the melted matter, nor to leave ore and stones below them unmelted; but the invention is very ingenious, and will be of immense service if it can be perfected; for in many places iron ore is to be found where water cannot be had for working a blast furface.

The last application which we shall make of the currents produced by heating the air is to the freeing mines, ships, prisons, &c. from the damp and noxious vapours which frequently infest them.

As a drift or work is carried on in the mine, let a trunk of deal boards, about six or eight inches square, be laid along the bottom of the drift, communicating with a trunk carried up in the corner of one of the shafts. Let the top of this last trunk open into the ash-pit of a small furnace, having a tall chimney. Let fire be kindled in the furnace; and when it is well heated, shut the fire-place and ash-pit doors. There being no other supply for the current produced in the chimney of this furnace, the air will flow into it from the trunk, and will bring along with it all the offensive vapours. This is the most effectual method yet found out. In the same manner may trunks be conducted into the ash-pit of a furnace from the cells of a prison or the wards of an hospital.

In the account which we have been giving of the management of air in furnaces and common fires, we have frequently mentioned the immediate application of air to the burning fuel as necessary for its combustion. This is a general fact. In order that any inflammable body may be really inflamed, and its combustible matter consumed and ashes produced, it is not enough that the body be made hot. A piece of charcoal enclosed in a box of iron may be kept red-hot for ever, without wasting its substance in the smallest degree. It is farther necessary that it be in contact with a particular species of air, which constitutes about three-eighths of the air of the atmosphere; viz. the vital air of Lavoisier. It was called *empyreal air* by Scheele, who first observed its indispensable use in maintaining fire: and it appears, that in contributing to the combustion of an inflammable body, this air combines with some of its ingredients, and becomes fixed air, suffering the same change as by the breathing of animals. Combustion may therefore be considered as a solution of the inflammable body in air. This doctrine was first promulgated by the celebrated Dr Hooke in his *Micographia*, published in 1660, and afterwards improved in his Treatise on Lamps. It is now completely established, and considered as a new discovery It is for this reason that in fire-places of all kinds we have directed the construction, so as to produce a close application of the air to the fuel. It is quite needless at this day to enter into the discussions which formerly occupied philosophers about the manner in which the pressure and elasticity of the air promoted combustion. Many experiments were made in the last century by the first members of the Royal Society, to discover the office of air in combustion. It was thought that the flame was extinguished in rare air for want of a pressure to keep it together; but this did not explain its extinction when the air was not renewed. These experiments are still retained in courses of experimental philosophy, as they are injudiciously styled; but they give little or no information, nor tend to the illustration of any pneumatical doctrine; they are therefore omitted in this place.

In short, it is now fully established, that it is not a mechanical but a chemical phenomenon. We can only inform the chemist, that a candle will consume faster in the Low Countries than in the elevated regions of Quito and Gondar, because the air is nearly one-half denser below, and will act proportionally faster in decomposing the candle.

We shall conclude this part of our subject with the explanation of a curious phenomenon observed in many places. Certain springs or fountains are observed to have periods of repletion and scantiness, or seem to ebb and flow at regular intervals; and some of these periods are of a complicated nature. Thus a well will have several returns of high and low water, the difference of which gradually increases to a maximum, and then diminishes, just as we observe in the ocean. A very ingenious and probable explanation of this has been given in No 424 of the Philosophical Transactions, by Mr Atwell, as follows:

Let ABCD (Fig. 80.) represent a cavern, into which water is brought by the subterraneous passage OT. Let it have an outlet MNP, of a crooked form, with its highest part N considerably raised above the bottom of the cavern, and thence sloping downwards into lower ground, and terminating in an open well at P. Let the dimensions of this canal be such that it will discharge much more water than is supplied by TO. All this is very natural, and may be very common. The effect of this arrangement will be a remitting spring at P: for when the cavern is filled higher than the point N, the canal MNP will act as a syphon; and, by the conditions assumed, it will discharge the water faster than TO supplies it; it will therefore run it dry, and then the spring at P will cease to furnish water. After some time the cavern will again be filled up to the height N, and the flow at P will recommence.

If, besides this supply, the well P also receive water from a constant source, we shall have a reciprocating spring.

The situation and dimensions of this syphon canal, and the supply of the feeder, may be such, that the efflux at P will be constant. If the supply increase in a certain degree, a reciprocation will be produced at P with very short intervals; if the supply diminishes considerably, we shall have another kind of reciprocation with great intervals and great differences of water.

If the cavern has another simple outlet R, new varieties will be produced in the spring P, and R will afford a curious spring. Let the mouth of R, by which the water enters it from the cavern, be lower than N, and let the supply of the feeding spring be no greater than R can discharge, we shall have a constant spring from R, and P will give no water. But suppose that the main feeder increases in winter or in rainy seasons, but not so much as will supply both P and R, the cavern will fill till the water gets over N, and R will be running all the while; but soon after P has begun to flow, and the water in the cavern sinks below R, the stream from R will stop. The cavern will be emptied by the syphon canal MNP, and then P will stop. The cavern will then begin to fill, and when near full R will give a little water, and soon after P will run and R stop as before, &c.

Desaguliers shows, vol. ii. p. 177, &c. in what manner a prodigious variety of periodical ebbs and flows may be produced by underground canals, which are extremely simple and probable.

We shall conclude this article with the descriptions of some pneumatical machines or engines which have not been particularly noticed under their names in the former volumes of this work.

Bellows are of most extensive and important use; and it will be of service to describe such as are of uncommon construction and great power, fit for the great operations in metallurgy.

It is not the impulsive force of the blast that is wanted in most cases, but merely the copious supply of air, to produce the rapid combustion of inflammable matter; and the service would be better performed in general if this could be done with moderate velocities, and an extended surface. What are called air-furnaces, where a considerable surface of inflammable matter is acted on at once by the current which the mere heat of the expended air has produced, are found more operative in proportion to the air expended than blast furnaces animated by bellows; and we doubt not but that the method proposed by M. Cotterel (which we have already mentioned) of increasing this current in a melting furnace by means of a dome, will in time supersede the blast furnaces. There is indeed a great impulsive force required in some cases; as for blowing off the scoriæ from the surface of silver or copper in refining furnaces, or for keeping a clear passage for the air in the great iron furnace.

In general, however, we cannot procure this abundant supply of air any other way than by giving it a great velocity by means of a great pressure, so that the general construction of bellows is pretty much the same in all kinds. The air is admitted into a very large cavity, and then expelled from it through a small hole.

The furnaces at the mines having been greatly enlarged, it was necessary to enlarge the bellows also: and the leathern bellows becoming exceedingly expensive, wooden ones were substituted in Germany about the beginning of last century, and from them became general through Europe. They consist of a wooden box ABCPFE (Fig. 92.), which has its top and two sides flat or straight, and the end BAE *e* formed into an arched or cylindrical surface, of which the line FP at the other end is the axis. This box is open below, and receives within it the shallow box KHGNML (Fig. 93), which exactly fills it. The line FP of the one coincides with FP of the other, and along this line is a set of hinges on which the upper box turns as it rises and sinks. The lower box is made fast to a frame fixed in the ground. A pipe OQ proceeds from the end of it, and terminates at the furnace, where it ends in a small pipe called the *tewer* or *tuyere*. This lower box is open above, and has in its bottom two large valves V, V, opening inwards. The conducting pipe is sometimes furnished with a valve opening outwards, to prevent burning coals from being sucked into the bellows when the upper box is drawn up. The joint along PF is made tight by thin leather nailed along it. The sides and ends of the fixed box are made to fit the sides and curved end of the upper box, so that this last can be raised and lowered round the joint FP without sensible friction, and yet without suffering much air to escape: but as this would not be sufficiently air-tight by reason of the shrinking and warping of the wood, a farther contrivance is adopted. A slender lath of wood, divided into several joints, and covered on the outer edge with very soft leather, is laid along the upper edges of the sides and ends of the lower box. This lath is so broad, that when its inner edge is even with the inside of the box, its outer edge projects about an inch. It is kept in this position by a number of steel wires, which are driven into the bottom of the box, and stand up touching the sides, as represented in Fig. 95, where *a b c* are the wires, and *e* the lath, projecting over the outside of the box. By this contrivance the laths are pressed close to the sides and curved end of the moveable box, and the spring wires yield to all their inequalities. A bar of wood RS is fixed to the upper board, by which it is either raised by machinery, to sink again by its own weight, having an additional load laid on it, or it is forced downward by a crank or wiper of the machinery, and afterwards raised.

The operation here is precisely similar to that of blowing with a chamber-bellows. When the board is lifted up, the air enters by the valves V, V, Fig. 94. and is expelled at the pipe OQ by depressing the boards. There is therefore no occasion to insist on this point.

These bellows are made of a very great size, AD being 16 feet, AB five feet, and the circular end AE also five feet. The rise, however, is but about 3 or 3½ feet. They expel at each stroke about 90 cubic feet of air, and they make about 8 strokes per minute.

Such are the bellows in general use on the continent. We have adopted a different form in this kingdom, which seems much preferable. We use an iron or wooden cylinder, with a piston sliding along it. This may be made with much greater accuracy than the wooden boxes, at less expense, if of wood, because it may be of coopers work, held together by hoops; but the great advantage of this form is its being more easily made air-tight. The piston is surrounded with a broad strap of thick and soft leather, and it has around its edge a deep groove, in which is lodged a quantity of wool. This is called the packing or stuffing, and keeps the leather very closely applied to the inner surface of the cylinder. Iron cylinders may be very neatly bored and smoothed, so that the piston, even when very tight, will slide along it very smoothly. To promote this, a quantity of black-lead is ground very fine with water, and a little of this is smeared on the inside of the cylinder from time to time.

The cylinder has a large valve, or sometimes two in the bottom, by which the atmospheric air enters when the piston is drawn up. When the piston is thrust down, this air is expelled along a pipe of great diameter, which terminates in the furnace with a small orifice.

This is the simplest form of bellows which can be conceived. It differs in nothing but size from the bellows used by the rudest nations. The Chinese smiths have a bellows very similar, being a square pipe of wood ABCDE (Fig. 96.), with a square board G which exactly fits it, moved by the handle FG. At the farther end is the blast pipe HK, and on each side of it a valve in the end of the square pipe, opening inwards. The piston is sufficiently tight for their purposes without any leathering.

The piston of this cylinder bellows is moved by machinery. In some blast engines the piston is simply raised by the machine, and then let go, and it descends by its own weight, and compresses the air below it to such a degree, that the velocity of efflux becomes constant, and the piston descends uniformly: for this purpose it must be loaded with a proper weight. This produces a very uniform blast, except at the very beginning, while the piston falls suddenly and compresses the air: but in most engines the piston rod is forced down the cylinder with a determined motion, by means of a beam, crank, or other contrivance. This gives a more unequal blast, because the motion of the piston is necessarily slow in the beginning and end of the stroke, and quicker in the middle.

But in all it is plain that the blast must be desultory. It ceases while the piston is rising; for this reason it is usual to have two cylinders, as it was formerly usual to have two bellows which worked alternately. Sometimes three or four are used, as at the Carron iron-works. This makes a blast abundantly uniform.

But an uniform blast may be made with a single cylinder, by making it deliver its air into another cylinder, which has a piston exactly fitted to its bore, and loaded with a sufficient weight. The blowing cylinder ABCD (Fig. 97.) has its piston P worked by a rod NP, connected by double chains, with the arched head of the working beam NO moving round a gudgeon at R. The other end O of this beam is connected by the rod OP, with the crank PQ of a wheel machine; or it may be connected with the piston of a steam-engine, &c. &c. The blowing cylinder has a valve or valves E in its bottom, opening inwards. There proceeds from it a large pipe CF, which enters the regulating cylinder GHKI, and has a valve at top to prevent the air from getting back into the blowing cylinder. It is evident that the air forced into this cylinder must raise its piston L, and that it must afterwards descend, while the other piston is rising. It must descend uniformly, and make a perfectly equable blast.

Observe, that if the piston L be at the bottom when the machine begins to work, it will be at the bottom at the end of every stroke, if the *tuyere* T emits as much air as the cylinder ABCD furnishes; nay, it will lie a while at the bottom; for, while it was rising, air was issuing through T. This would make an interrupted blast. To prevent this, the orifice T must be lessened; but then there will be a surplus of air at the end of each stroke, and the piston L will rise continually, and at last get to the top, and allow air to escape. It is just possible to adjust circumstances, so that neither shall happen. This is done easier by putting a stop in the way of the piston, and putting a valve on the piston, or on the conducting pipe KST, loaded with a weight a little superior to the intended elasticity of the air in the cylinder. Therefore when the piston is prevented by the stop from rising, the snifting valve, as it is called, is forced open, the superfluous air escapes, and the blast preserves its uniformity.

It may be of use to give the dimensions of a machine of this kind, which has worked for some years at a very great furnace, and given satisfaction.

The diameter of the blowing cylinder is 5 feet, and the length of the stroke is 6. Its piston is loaded with 3½ tons. It is worked by a steam-engine whose cylinder is 3 feet 4 inches wide, with a six-feet stroke. The regulating cylinder is 8 feet wide, and its piston is loaded with 8½ tons, making about 2,63 pounds on the square inch; and it is very nearly in equilibrio with the load on the piston of the blowing cylinder. The conducting pipe KST is 12 inches in diameter, and the orifice of the tuyere was 1⅝ inches when the engine was erected, but it has gradually enlarged by reason of the intense heat to which it is exposed. The snifting valve is loaded with three pounds on the square inch.

When the engine worked briskly, it made 18 strokes per minute, and there was always much air discharged by the snifting valve. When the engine made 15 strokes per minute, the snifting valve opened but seldom, so that things were nearly adjusted to this supply. Each stroke of the blowing cylinder sent in 118 cubic feet of common air. The ordinary pressure of the air being supposed 14¾ pounds on an inch, the density of the air in the regulating cylinder must be $\frac{14,75+2,63}{14,75}$, $=1,1783$, the natural density being 1.

This machine gives an opportunity of comparing the expence of air with the theory. It must (at the rate of 15 strokes) expel 30 cubic feet of air in a second through a hole of 1⅝ inches in diameter. This gives a velocity of near 2000 feet per second, and of more than 1600 feet for the condensed air. This is vastly greater than the theory can give or is indeed possible; for air does not rush into a void with so great velocity. It shows with great evidence, that a vast quantity of air must escape round the two pistons. Their united circumferences amount to above 40 feet, and they move in a dry cylinder. It is impossible to prevent a very great loss. Accordingly, a candle held near the edge of the piston L has its flame very much disturbed. This case, therefore, gives no hold for a calculation; and it suggests the propriety of attempting to diminish this great waste.

This has been very ingeniously done (in part at least) at some other furnaces. At Omoah foundry, near Glasgow, the blowing cylinder (also worked by a steam-engine) delivers its air into a chest without a bottom, which is immersed in a large cistern of water, and supported at a small height from the bottom of the cistern, and has a pipe from its top leading to the tuyere. The water stands about five feet above the lower brim of the regulating air-chest, and by its pressure gives the most perfect uniformity of blast, without allowing a particle of air to get off by any other passage besides the tuyere. This is a very effectual regulator, and must produce a great saving of power, because a smaller blowing cylinder will thus supply the blast. We have not learned the dimensions and performance of this engine. We must observe, that the loss round the piston of the blowing cylinder remains undiminished.

A blowing machine was erected many years ago at Chastillon, in France, on a principle considerably different, and which must be perfectly air-tight throughout. Two cylinders A, B (Fig. 98.) loaded with great weights, were suspended at the ends of the lever CD, moving round the gudgeon E. From the top F, G of each there was a large flexible pipe which united in H, from whence a pipe KT led to the tuyere T. There were valves at F and G opening outwards, or into the flexible pipes; and other valves L, M, adjoining to them in the top of each cylinder opening inwards, but kept shut by a slight spring. Motion was given to the lever by a machine. The operation of this blowing machine is evident. When the cylinder A was pulled down, or allowed to descend, the water, entering at its bottom, compressed the air, and forced it along the passage FHKT. In the meantime, the cylinder B was rising, and the air entered by the valve M. We see that the blast will be very unequal, increasing as the cylinder is immersed deeper. It is needless to describe this machine more particularly, because we shall give an account of one which we think perfect in its kind, and which leaves hardly any thing to be desired in a machine of this sort.

It was invented by Mr John Laurie, land-surveyor in Edinburgh, many years ago, and improved in some respects since his death by an ingenious person of that city.

ABCD (Fig. 99.) is an iron cylinder, truly bored within, and evasated a-top like a cup. EFGH is another, truly turned both without and within, and a small matter less than the inner diameter of the first cylinder. This cylinder is close above, and hangs from the end of a lever moved by a machine. It is also loaded with weights at N. KILM is a third cylinder, whose outside diameter is somewhat less than the inside diameter of the second. This inner cylinder is fixed to the same bottom with the outer cylinder. The middle cylinder is loose, and can move up and down between the outer and inner cylinders, without rubbing on either of them. The inner cylinder is perforated from top to bottom by three pipes OQ, SV, PR. The pipes OQ, PR have valves at their upper ends O, P, and communicate with the external air below. The pipe SV has a horizontal part VW, which again turns upwards, and has a valve at top X. This upright part WX is in the middle of a cistern of water *f b k g*. Into this cistern is fixed an air-chest *a* YZ *b*, open below, and having at top a pipe *c d e* terminating in the tuyere at the furnace.

When the machine is at rest, the valves X, O, P, are shut by their own weights, and the air-chest is full of water. When things are in this state, the middle cylinder EFGH is drawn up by the machinery till its lower brims F and G are equal with the top RM of the inner cylinder. Now pour in water or oil between the outer and middle cylinders: it will run down and fill the space between the outer and inner cylinders. Let it come to the top of the inner cylinder.

Now let the loaded middle cylinder descend. It cannot do this without compressing the air which is between its top and the top of the inner cylinder. This air being compressed will cause the water to descend between the inner and middle cylinders, and rise between the middle and outer cylinders, spreading into the cup; and as the middle cylinder advances downwards, the water will descend farther within it and rise farther without it. When it has got so far down, and the air has been so much compressed, that the difference between the surface of the water on the inside and outside of this cylinder is greater than the depth of water between X and the surface of the water *f g*, air will go out by the pipe SVW, and will lodge in the air chest, and will remain there if *c* be shut, which we shall suppose for the present. Pushing down the middle cylinder till the partition touch the top of the inner cylinder, all the air which was formerly between them will be forced into the air-chest, and will drive out water from it. Draw up the middle cylinder, and the external air will open the valves O, P, and again fill the space between the middle and inner cylinders; for the valve X will shut, and prevent the regress of the condensed air. By pushing down the middle cylinder a second time, more air will be forced into the air-chest, and it will at last escape by getting out between its brims Y, Z, and the bottom of the cistern; or if we open the passage *c*, it will pass along the conduit *c d e* to the tuyere, and form a blast.

The operation of this machine is similar to Mr Haskins's quicksilver pump described by Desaguliers at the end of the second volume of his Experimental Philosophy. The force which condenses the air is the load on the middle cylinder. The use of the water between the inner and outer cylinders is to prevent this air from escaping; and the inner cylinder thus performs the office of a piston, having no friction. It is necessary that the length of the outer and middle cylinders be greater than the depth of the regulator-cistern, that there may be a sufficient height for the water to rise between the middle and outer cylinders, to balance the compressed air, and oblige it to go into the air-chest. A large blast-furnace will require the regulator-cistern five feet deep, and the cylinders about six or seven feet long.

It is in fact a pump without friction, and is perfectly air-tight. The quickness of its operation depends on the small space between the middle cylinder and the two others; and this is the only use of these two. Without these it would be similar to the engine at Chastillon, and operate more unequally and slowly. Its only imperfection is, that if the cylinder begin its motion of ascent or descent rapidly, as it will do when worked by a steam-engine, there will be some danger of water dashing over the top of the inner cylinder and getting into the pipe SV; but should this happen, an issue can easily be contrived for it at V, covered with a loaded valve *v*. This will never happen if the cylinder is moved by a crank.

One blowing cylinder only is represented here, but two may be used.

We do not hesitate in recommending this form of bellows as the most perfect of any, and fit for all uses where standing bellows are required. They will be cheaper than any other sort fit for common purposes. For a common smith's forge they may be made with square wooden boxes instead of cylinders. They are also easily repaired. They are perfectly tight; and they may be made with a blast almost perfectly uniform, by making the cistern in which the air-chest stands of considerable dimensions. When this is the case, the height of water, which regulates the blast, will vary very little.

This may suffice for an account of blast machines. The leading parts of their construction have been described as far only as was necessary for understanding their operation, and enabling an engineer to erect them in the most commodious manner. Views of complete machines might have amused, but they would not have added to our reader's information.

But the account is imperfect, unless we show how their parts may be so proportioned that they shall perform what is expected from them. The engineer should know what size of bellows, and what load on the board or piston, and what size of tuyere, will give the blast which the service requires, and what force must be employed to give them the necessary degree of motion. We shall accomplish these purposes by considering the efflux of the compressed air through the tuyere. The propositions formerly delivered will enable us to ascertain this.

That we may proportion every thing to the power employed, we must recollect, that if the piston of a cylinder employed for expelling air be pressed down with any force p, it must be considered as superadded to the atmospheric pressure P on the same piston, in order that we may compare the velocity v of efflux with the known velocity V with which air rushes into a void. By what has been formerly delivered, it appears that this velocity $v = V \times \sqrt{\frac{p}{P \cancel{\times} p}}$, +
where P is the pressure of the atmosphere on the piston, and p the additional load laid on it. This velocity is expressed in feet *per* second; and, when multiplied by the area of the orifice (also expressed in square feet), it will give us the cubical feet of condensed air expelled in a second; but the bellows are always to be filled again with common air, and therefore we want to know the quantity of common air which will be expelled; for it is this which determines the number of strokes which must be made in a minute, in order that the proper supply may be obtained. Therefore recollect that the quantity expelled from a given orifice with a given velocity, is in the proportion of the density; and that when D is the density of common air produced by the pressure P, the density d produced by the pressure $P+p$, is $D \times \frac{P+p}{P}$; or if D be made 1, we have $d = \frac{P+p}{P}$.

Therefore calling the area of the orifice expressed in square feet O, and the quantity of common air, or the cubic feet expelled in a second Q, we have $Q = V \times O \times \sqrt{\frac{p}{P+p}} \times \frac{P+p}{P}$.

It will be sufficiently exact for all practical purposes to suppose P to be 15 pounds on every square inch of the piston; and p is then conveniently expressed by the pounds of additional load on every square inch: we may also take $V = 1332$ feet.

As the orifice through which the air is expelled is generally very small, never exceeding three inches in diameter, it will be more convenient to express it in square inches; which being the $\frac{1}{144}$ of a square foot, we shall have the cubic feet of common air expelled in a second, or $Q = \frac{1332}{144} O \sqrt{\frac{p}{P+p}} \times \frac{P+p}{P}, = O \times 9{,}25 \times \sqrt{\frac{p}{P+p}} \times \frac{P+p}{P}$ and this seems to be as simple an expression as we can obtain.

This will perhaps be illustrated by taking an example in numbers. Let the area of the piston be four square feet, and the area of the round hole through which the air is expelled be two inches, its diameter being 1,6, and let the load on the piston be 1728 pounds: this is three pounds on every square inch. We have $P = 15$, $p = 3$, $P + p = 18$, and $O = 2$; therefore we will have $Q = 2 \times 9{,}25 \times \sqrt{\frac{3}{18}} \times \frac{18}{15}, = 9{,}053$ cubic feet of common air expelled in a second. This will however be diminished at least one-third by the contraction of the jet; and therefore the supply will not exceed six cubic feet *per* second. Supposing therefore that this blowing machine is a cylinder or prism of this dimension in its section, the piston so loaded would (after having compressed the air) descend about 15 inches in a second: it would first sink $\frac{1}{16}$ of the whole length of the cylinder pretty suddenly, till it had reduced the air to the density $\frac{16}{15}$, and would then descend uniformly at the above rate, expelling six cubic feet of common air in a second.

The computation is made much in the same way for bellows of the common form, with this additional circumstance, that as the loaded board moves round a hinge at one end, the pressure of the load must be calculated accordingly. The computation, however, becomes a little intricate, when the form of the loaded board is not rectangular; it is almost useless when the bellows have flexible sides, either like smith's bellows or like organ bellows, because the change of figure during their motion makes continual variation on the compressing powers. It is therefore chiefly with respect to the great wooden bellows, of which the upper board slides down between the sides, that the above calculation is of service.

The propriety however of this piece of information is evident; we do not know precisely the quantity of air necessary for animating a furnace; but this calculation *tells* us what force must be employed for expelling the air that may be thought necessary. If we have fixed on the strength of the blast, and the diameter of the cylinder, we learn the weight with which the piston must be loaded; the length of the cylinder determines its capacity, the above calculation tells the expense *per* second; hence we have the time of the piston's coming to the bottom. This gives us the number of strokes *per* minute.: the load must be lifted up by the machine this number of times, making the time of ascent precisely equal to that of descent; otherwise the machine will either catch and stop the descent of the piston, or allow it to lie inactive for a while of each stroke. These circumstances determine the labour to be performed by the machine, and it must be constructed accordingly. Thus the engineer will not be affronted by its failure, nor will he expend needless power and cost.

In machines which force the piston or bellows-board with a certain determined motion, different from what arises from their own weight, the computation is extremely intricate. When a piston moves by a crank, its motion at the beginning and end of each stroke is slow, and the compression and efflux is continually changing: we can however approximate to a statement of the force required.

Every time the piston is drawn up, a certain space of the cylinder is filled again with air of the common density; and this is expelled during the descent of the piston. A certain number of cubic feet of common air is therefore expelled with a velocity which perhaps continually varies; but there is a medium velocity with which it might have been uniformly expelled, and a pressure corresponding to this velocity. To find this, divide the area of the piston by the area of the blast-hole (or rather by this area multiplied by 0,618, in order to take in the effect of the contracted jet), and multiply the length of the stroke performed in a second by the quotient arising from this division; the product is the medium velocity of the air (of the natural density). Then find by calculation the height through which a heavy body must fall in order to acquire this velocity; this is the height of a column of homogeneous air which would expel it with this velocity. The weight of this column is the least force that can be exerted by the engine: but this force is too small to overcome the resistance in the middle of the stroke, and it is too great even for the end of the stroke, and much too great for the beginning of it. But if the machine is turned by a very heavy water-wheel, this will act as a regulator, accumulating in itself the superfluous force during the too favourable positions of the crank, and exerting it by its *vis insita* during the time of greatest effort. A force not greatly exceeding the weight of this column of air will therefore suffice. On the other hand, if the strength of the blast be determined, which is the general state of the problem, this determines the degree of condensation of the air, and the load on the square inch of the piston, or the mean force which the machine must exert on it. A table, which will be given presently, determines the cubic feet of common air expelled in a second, corresponding to this load. This combined with the proposed dimensions of the cylinder, will give the descent of the piston or the length of the stroke.

These general observations apply to all forms of bellows; and without a knowledge of them no person can erect a machine for working them without total uncertainty or servile imitation. In order, therefore, that they may be useful to such as are not accustomed to the management of even these simple formulæ, we insert the following short table of the velocity and quantity of air discharged from a cylinder whose piston is loaded with the pounds contained in the first column on every square inch. The second column contains the velocity with which the *condensed air* rushes out through any *small* hole; and the third column is the cubic feet discharged from a hole whose area is a square inch; column fourth contains the mean velocity of air of the common density; and column fifth is the cubic feet of common air discharged; the sixth column is the height in inches at which the force of the blast would support a column of water if a pipe were inserted into the side of the cylinder. This is an extremely proper addition to such mechanics, showing at all times the power of the machines, and teaching us what intensity of blast is employed for different purposes. The table is computed from the supposition that the ordinary pressure of the air is 15 pounds on a square inch. This is somewhat too great, and therefore the velocities are a little too small; but the quantities discharged will be found about ⅓ too great (without affecting the velocities) on account of the convergency of the stream.

I	II	III	IV	V	VI
½	239	1,66	247	1,72	14
1	333	2,31	355	2,47	27
1½	404	2,79	437	3,05	40
2	457	3,17	518	3,60	54
2½	500	3,48	584	4,2	68
3	544	3,76	653	4,53	82
3½	582	4,03	715	4,98	95
4	611	4,24	774	5,38	109
4½	642	4,46	822	5,75	122
5	666	4,67	888	6,17	136
5½	693	4,84	950	6,49	150
6	711	5,06	997	6,92	163

This table extends far beyond the limits of ordinary use, very few blast-furnaces having a force exceeding 60 inches of water.

We shall conclude this account of blowing machines with a description of a small one for a blow-pipe. (Fig. 100.) EFGH is a vessel containing water, about two feet deep. ABCD is the air-box of the blower open below, and having a pipe ILK rising up from it to a convenient height; an arm ON which grasps this pipe carries the lamp N: the blow-pipe LM comes from the top of the upright pipe. PKQ is the feeding pipe reaching near to the bottom of the vessel.

Water being poured into the vessel below, and its cover being put on, which fits the upright pipe, and touches two studs *a*, *a*, projecting from it, blow in a quantity of air by the feeding pipe PQ; this expels the water from the air-box, and occasions a pressure which produces the blast through the blow-pipe M.

In a former paragraph of this article, we mentioned an application which has been made of Hero's fountain, at Chemnitz, in Hungary, for raising water from the bottom of a mine. We shall now give an account of this very ingenious contrivance.

In Fig. 101. B represents the source of water elevated above the mouth of the pit 136 feet. From this there is led a pipe B*b*CDE four inches diameter. This pipe enters the top of a copper cylinder *b c d e*, 8½ feet high, five feet diameter, and two inches thick, and it reaches to within four inches of the bottom; it has a cock at C. This cylinder has a cock at F, and a very large one at E. From the top *b c* proceeds a pipe GHH′ two inches in diameter, which goes down the pit 96 feet, and is inserted into the top of another brass cylinder *f g h i*, which is 6½ feet high, four feet diameter, and two inches thick, containing 83 cubic feet, which is very nearly one half of the capacity of the other, viz. of 170 cubic feet. There is another pipe NI of four inches diameter, which rises from within four inches of the bottom of this lower cylinder, is soldered into its top, and rises to the trough NO, which carries off the water from the mouth of the pit. This lower cylinder communicates at the bottom with the water L which collects in the drains of the mine. A large cock K serves to admit or exclude this water; another cock M, at the top of this cylinder, communicates with the external air.

Now suppose the cock C shut, and all the rest open; the upper cylinder will contain air, and the lower cylinder will be filled with water, because it is sunk so deep that its top is below the usual surface of the mine-waters. Now shut the cocks F, E, M, K, and open the cock C. The water of the source B must run in by the orifice D, and rise in the upper cylinder, compressing the air above it and along the pipe GHH′, and thus acting on the surface of the water in the lower cylinder. It will therefore cause it to rise gradually in the pipe IN, where it will always be of such a height that its weight balances the elasticity of the compressed air. Suppose no issue given to the air from the upper cylinder, it would be compressed into ⅕th of its bulk by the column of 136 feet high; for a column of 34 feet nearly balances the ordinary elasticity of the air. Therefore when there is an issue given to it through the pipe GHH, it will drive the compressed air along this pipe, and it will expel water from the lower cylinder. When the upper cylinder is full of water, there will be 84 cubic feet of water expelled from the lower cylinder. If the pipe IN had been more than 136 feet long, the water would have risen 136 feet, being then in equilibrio with the water in the feeding pipe BbCD (as has been already shown), by the intervention of the elastic air; but no more water would have been expelled from the lower cylinder than what fills this pipe. But the pipe being only 96 feet high, the water will be thrown out at N with a very great velocity. If it were not for the great obstructions which water and air must meet with in their passage along pipes, it would issue at N with a velocity of more than 50 feet per second. It issues much more slowly, and at last the upper cylinder is full of water, and the water would enter the pipe GH and enter the lower cylinder, and without displacing the air in it, would rise through the discharging pipe IN, and run off to waste. To prevent this there hangs in the pipe HG a cork ball or double cone, by a brass wire, which is guided by holes in two cross pieces in the pipe HG. When the upper cylinder is filled with water, this cork plugs up the orifice G, and no water is wasted; the influx at D now stops. But the lower cylinder contains compressed air, which would balance water in a discharging pipe 136 feet high, whereas IN is only 96. Therefore the water will continue to flow at N till the air has so far expanded as to balance only 96 feet of water, that is, till it occupies ¼ of its ordinary bulk, that is, ¼ of the capacity of the upper cylinder, or 42½ cubic feet. Therefore 42½ cubic feet will be expelled, and the efflux at N will cease; and the lower cylinder is about ½ full of water. When the attending workman observes this, he shuts the cock C. He might have done this before, had he known when the orifice G was stopped; but no loss ensues from the delay. At the same time the attendant opens the cock E, the water issues with great violence, being pressed by the condensed air from the lower cylinder. It therefore issues with the sum of its own weight and of this compression. These gradually decrease together, by the efflux of the water and the expansion of the air; but this efflux stops before all the water has flowed out; for there is 42¼ feet of the lower cylinder occupied by air. This quantity of water remains, therefore, in the upper cylinder nearly: the workman knows this, because the discharged water is received first of all into a vessel containing ¼ of the capacity of the upper cylinder. Whenever this is filled, the attendant opens the cock K by a long rod which goes down the shaft; this allows the water of the mine to fill the lower cylinder, allows the air to get into the upper cylinder, and this allows the remaining water to run out of it.

And thus every thing is brought into its first condition; and when the attendant sees no more water come out at E, he shuts the cocks E and M, and opens the cock C, and the operation is repeated.

There is a very surprising appearance in the working of this engine. When the efflux at N has stopped, if the cock F be opened, the water and air rush out together with prodigious violence, and the drops of water are changed into hail or lumps of ice. It is a sight usually shown to strangers, who are desired to hold their hats to receive the blast of air: the ice comes out with such violence as frequently to pierce the hat like a pistol bullet. This rapid congelation is a remarkable instance of the general fact, that air by suddenly expanding, generates cold, its capacity for heat being increased. Thus the peasant cools his broth by blowing over the spoon, even from warm lungs; a stream of air from a pipe is always cooling.

The above account of the procedure in working this engine shows that the efflux both at N and E becomes very slow near the end. It is found convenient therefore not to wait for the complete discharges, but to turn the cocks when about 30 cubic feet of water have been discharged at N: more work is done in this way. A gentleman of great accuracy and knowledge of these subjects took the trouble, at our desire, of noticing particularly the performance of the machine. He observed that each stroke, as it may be called, took up about three minutes and ¼; and that 32 cubic feet of water were discharged at N, and 66 were expended at E. The expense therefore is 66 feet of water falling 136 feet, and the performance is 32 raised 96, and they are in the proportion of 66×136 to 32×96, or of 1 to 0,3422, or nearly as 3 to 1. This is superior to the performance of the most perfect undershot mill, even when all friction and irregular obstructions are neglected; and is not much inferior to any overshot pump-mill that has yet been erected. When we reflect on the great obstructions which water meets with in its passage through long pipes, we may be assured that, by doubling the size of the feeder and discharger, the performance of the machine will be greatly improved; we do not hesitate to say, that it would be increased ⅓: it is true that it will expend more water; but this will not be nearly in the same proportion; for most of the deficiency of the machine arises from the needless velocity of the first efflux at N. The discharging pipe ought to be 110 feet high, and not give sensibly less water.

Then it must be considered how inferior in original expense this simple machine must be to a mill of any kind which would raise 10 cubic feet 96 feet high in a minute, and how small the repairs on it need be, when compared with a mill.

And, lastly, let it be noticed, that such a machine can be used where no mill whatever can be put in motion. A small stream of water, which would not move any kind of wheel, will here raise ⅓ of its own quantity to the same height; working as fast as it is supplied.

For all these reasons, we think that the Hungarian machine eminently deserves the attention of mathematicians and engineers, to bring it to its utmost perfection, and into general use. There are situations where this kind of machine may be very useful. Thus, where the tide rises 17 feet, it may be used for compressing air to $\frac{2}{3}$ of its bulk; and a pipe leading from a very large vessel inverted in it may be used for raising the water from a vessel of $\frac{1}{3}$ of its capacity 17 feet high; or if this vessel has only $\frac{1}{18}$ of the capacity of the large one set in the tide-way, two pipes may be led from it; one into the small vessel, and the other into an equal vessel 16 feet higher, which receives the water from the first. Thus $\frac{1}{18}$ of the water may be raised 34 feet, and a smaller quantity to a still greater height; and this with a kind of power that can hardly be applied in any other way. Machines of this kind are described by Schottus, Sturmius, Leupold, and other old writers; and they should not be forgotten, because opportunities may offer of making them highly useful. A gentleman's house in the country may thus be supplied with water by a machine that will cost little, and hardly go out of repair.

The last pneumatical engine which we shall speak of at present is the common fanners, used for winnowing grain, and for drawing air out of a room: and we have but few observations to make on them.

The wings of the fanners are enclosed in a cylinder or drum, whose circular sides have a large opening BDE (Fig. 102.) round the centre, to admit the air. By turning the wings rapidly round, the air is hurried round along with them, and thus acquires a centrifugal tendency, by which it presses strongly on the outer rim of the drum: this is gradually detached from the circle as at KI, and terminated in a trunk IHGF, which goes off in a tangential direction; the air therefore is driven along this passage.

If the wings were disposed in planes passing through the axis C, the compression of the air by their anterior surface would give it some tendency to escape in every direction, and would obstruct in some degree the arrival of more air through the side-holes. They are therefore reclined a little backward, as represented in the figure. It may be shown that their best form would be that of a hyperbolic spiral *a b c;* but the straight form approaches sufficiently near to the most perfect shape.

How much labour is lost, however, in carrying the air round those parts of the drum where it cannot escape. The fanners would either draw or discharge almost twice as much air if an opening were made all round one side. This could be gradually contracted (where required for winnowing) by a surrounding cone, and thus directed against the fallen grain: this has been verified by actual trial. When used for drawing air out of a room for ventilation, it would be much better to remove the outer side of the drum entirely, and let the air fly freely off on all sides; but the flat sides are necessary, in order to prevent the air from arriving at the fanners any other way but through the central holes, to which trunks should be fitted leading to the apartment which is to be ventilated.

END OF VOLUME THIRD.

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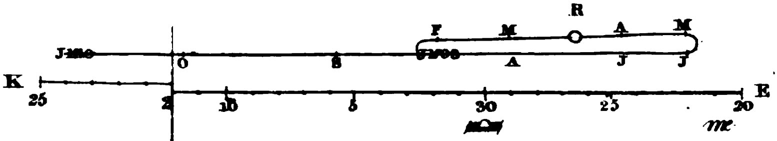

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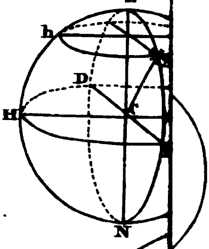

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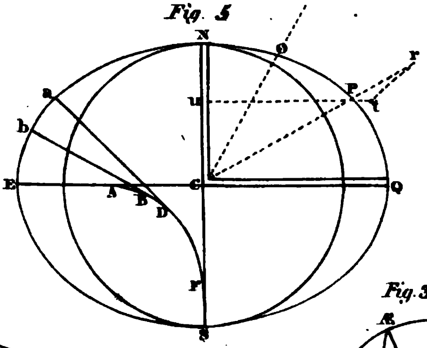

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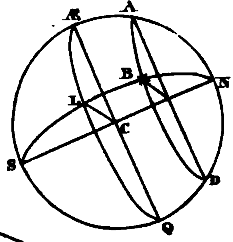

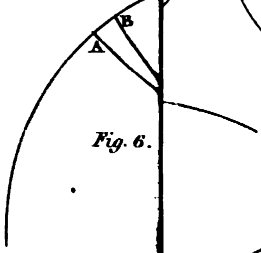

Fig. 6.

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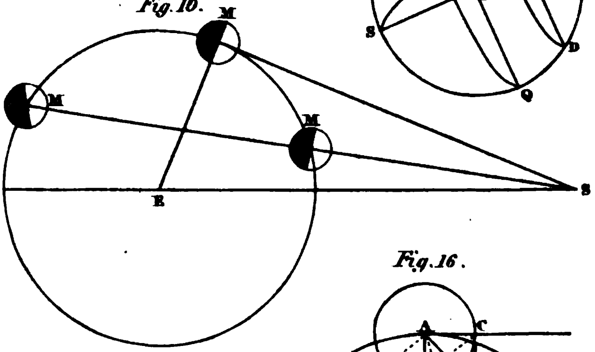

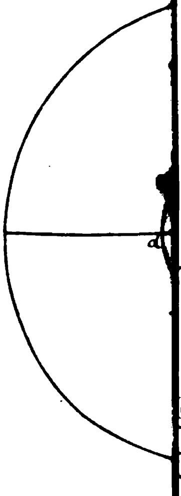

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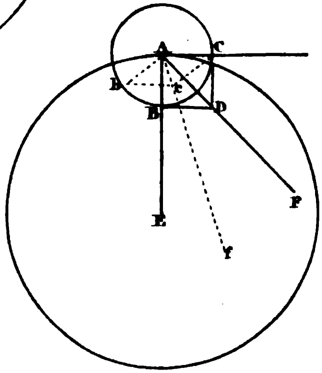

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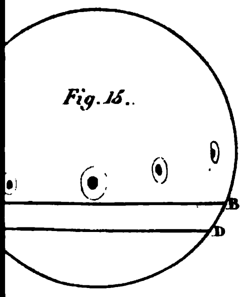

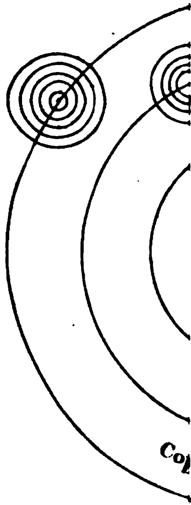

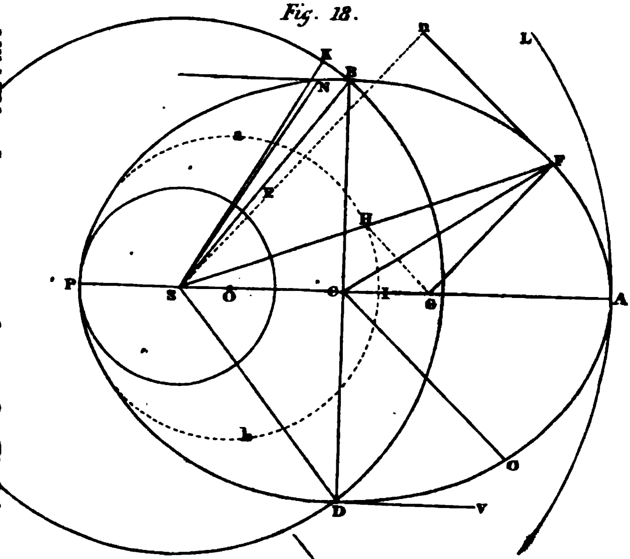
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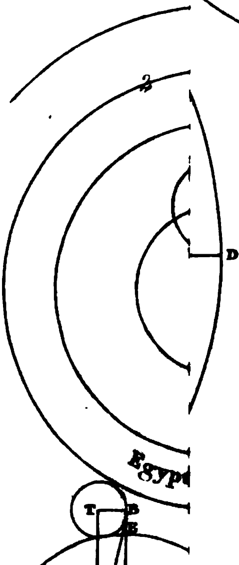

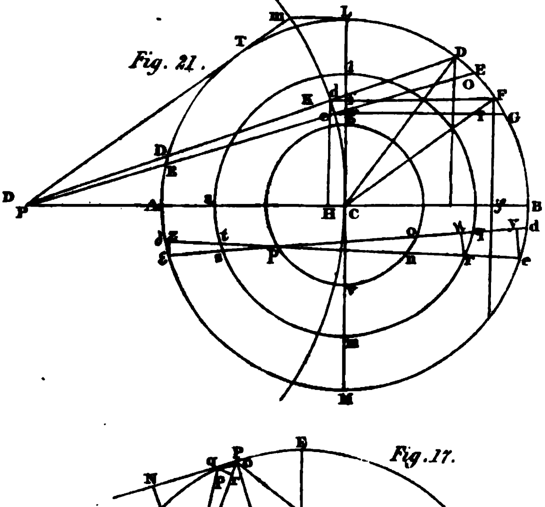
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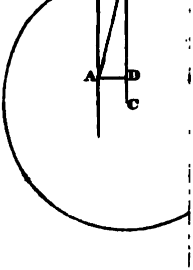

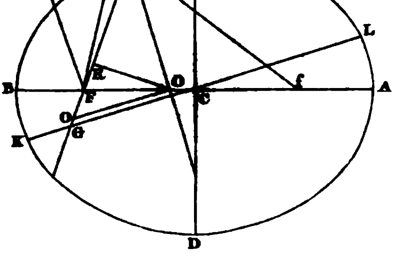
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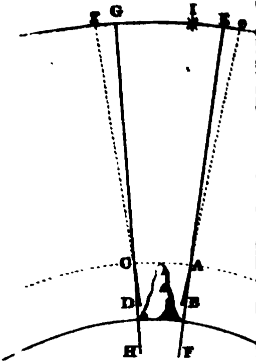

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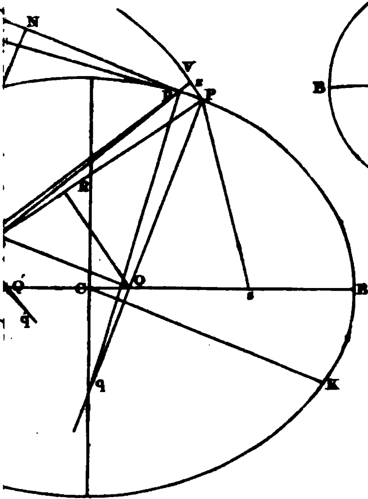

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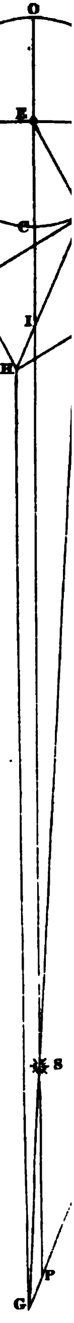

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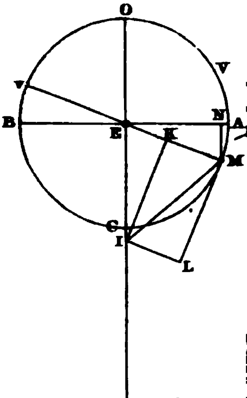

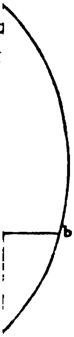

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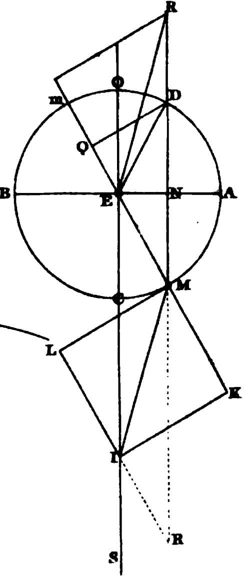

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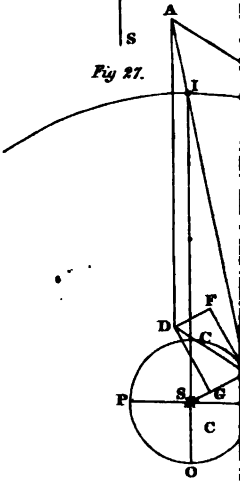

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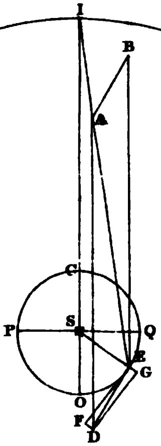
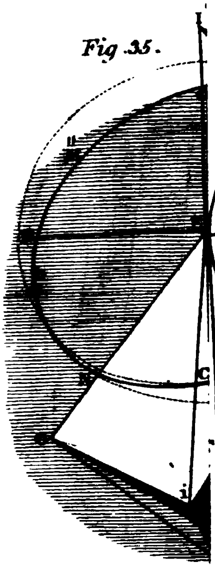


ASTRONOMY. *VOL. III PLATE IV.*

Fig. 35.



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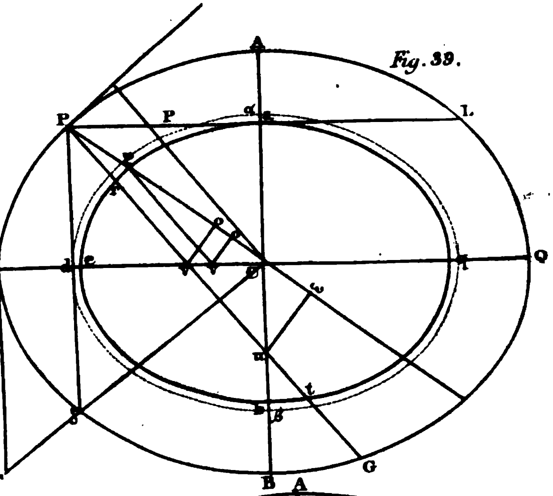

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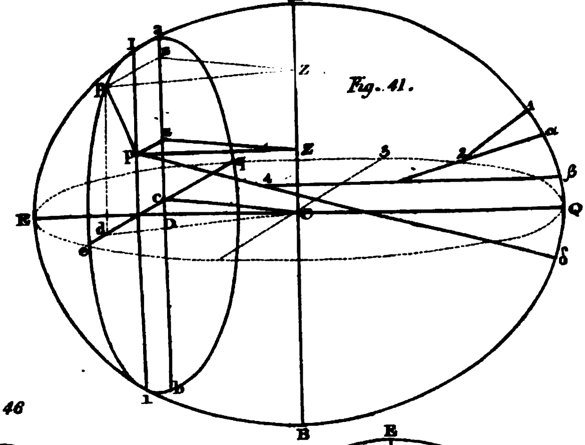

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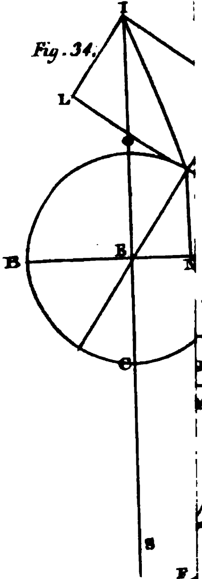

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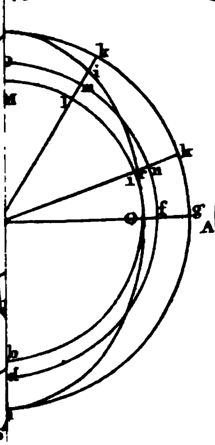

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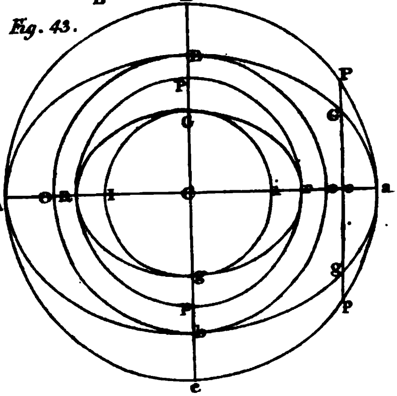

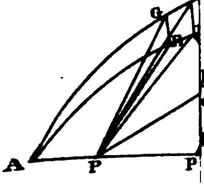

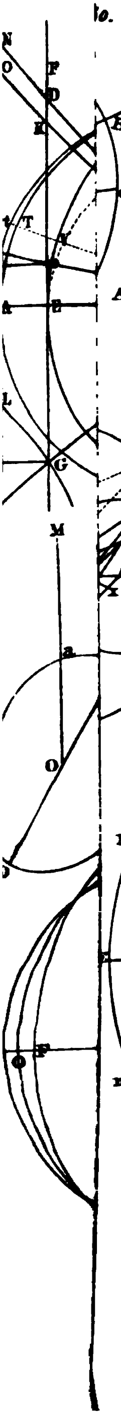
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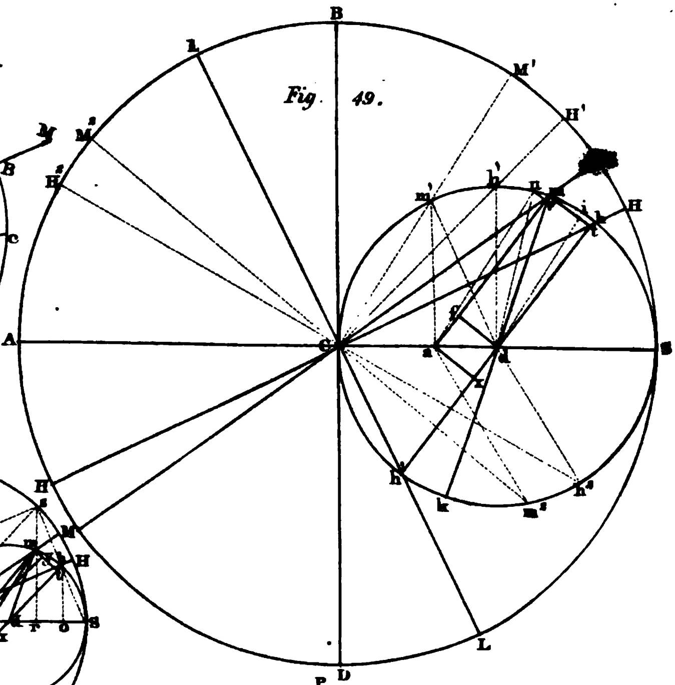
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Fig. 49.

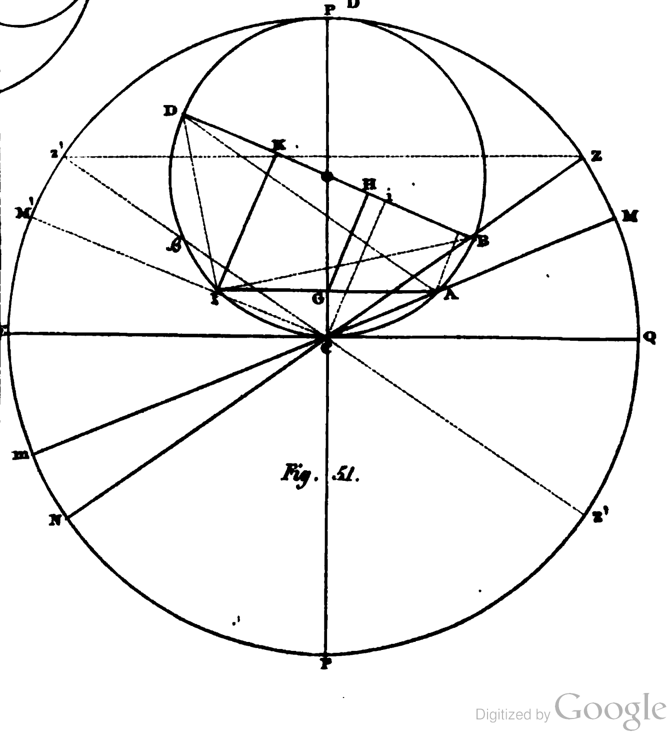
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Fig. 51.

TELESCOPE. VOL. III PLATE VI.

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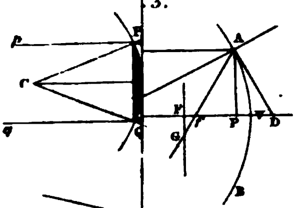

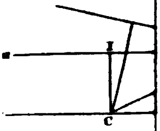

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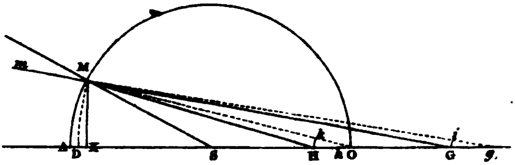

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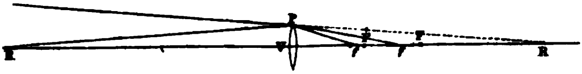

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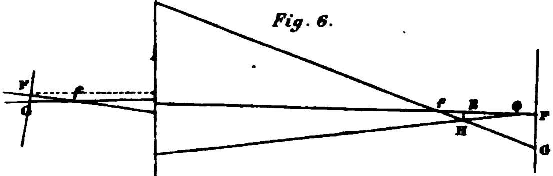

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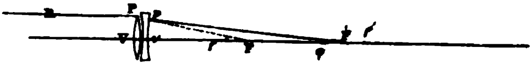

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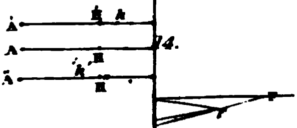

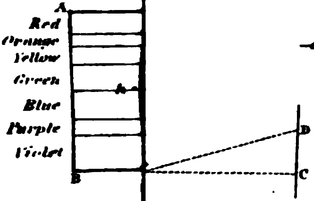

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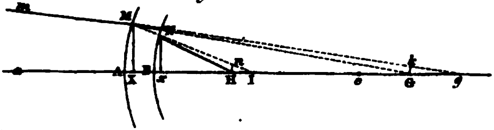

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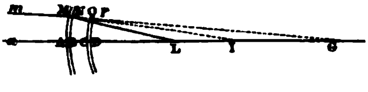

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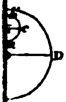
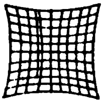


Fig. 18.



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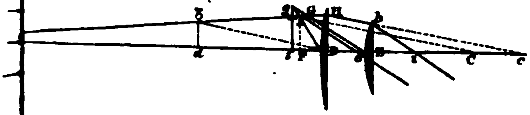
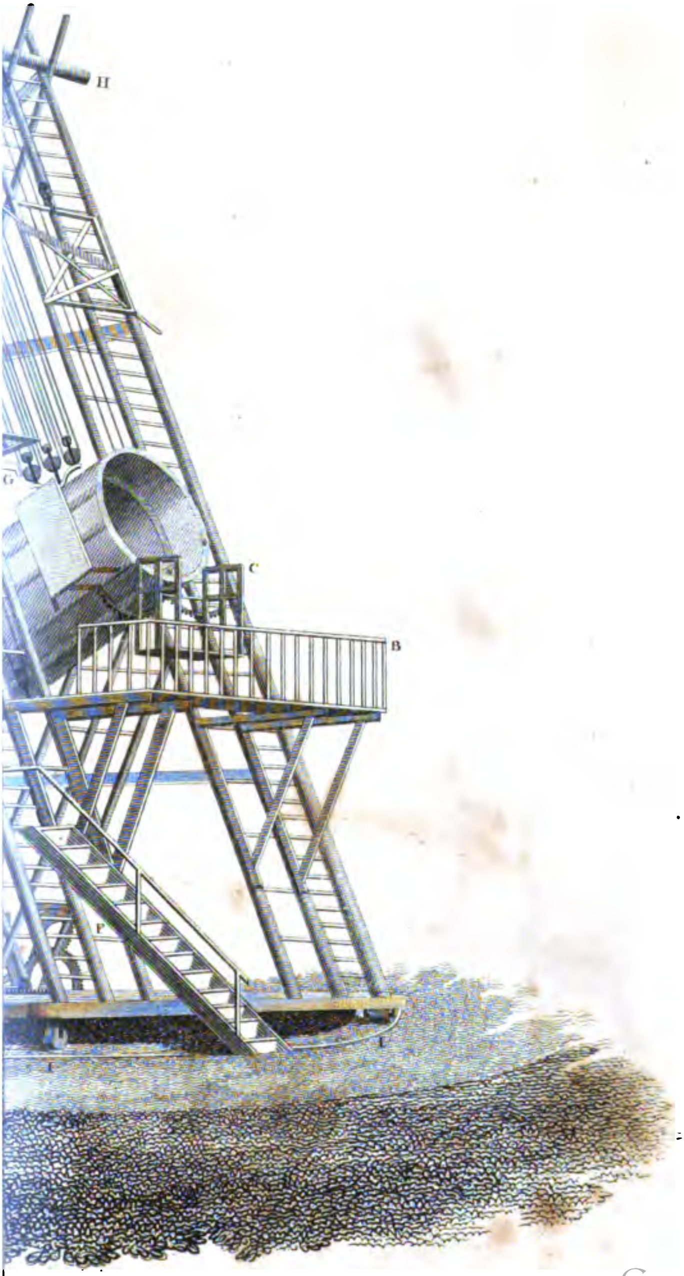
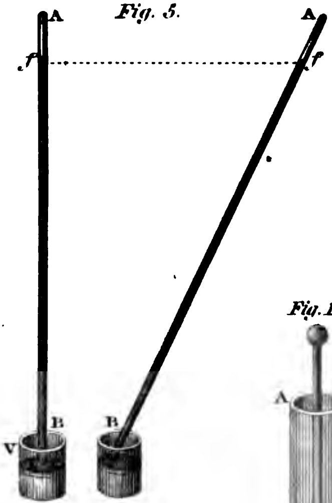


CHELS GREAT TELESCOPE. VOL. III. PLATE VIII.



PNEUMATICS VOL. III. PLATE IX.

Fig. 5.



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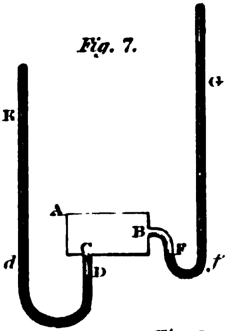

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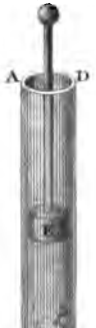

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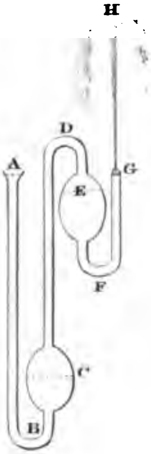

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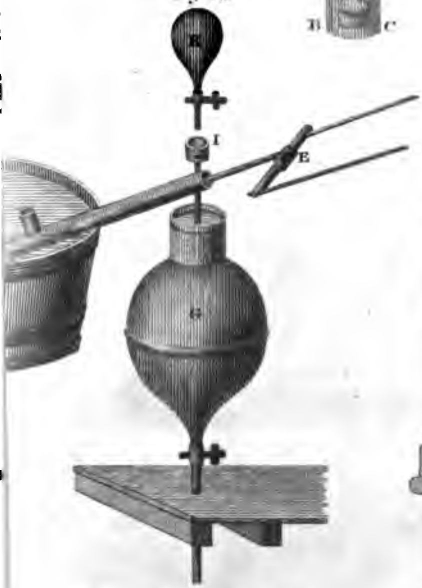

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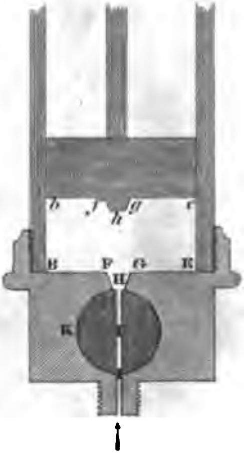

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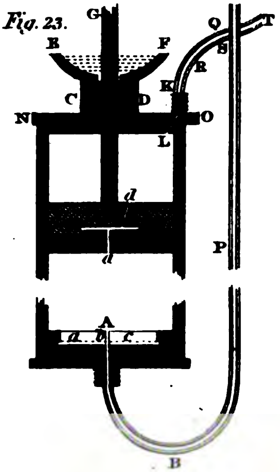
PNEUMATICS. VOL. III. PLATE X.

Fig. 23.



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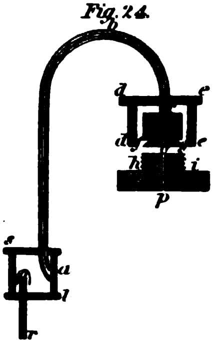

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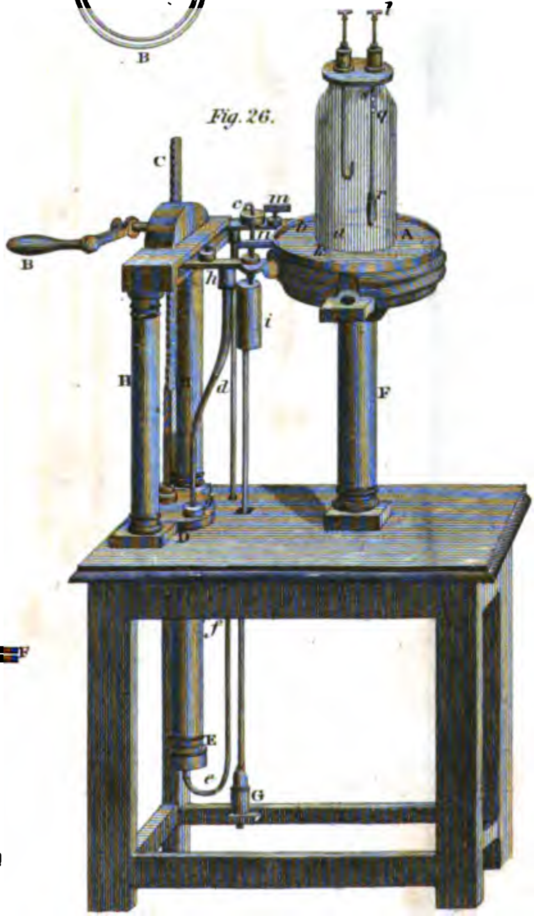

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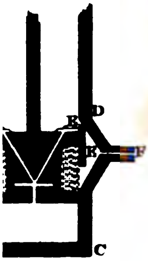
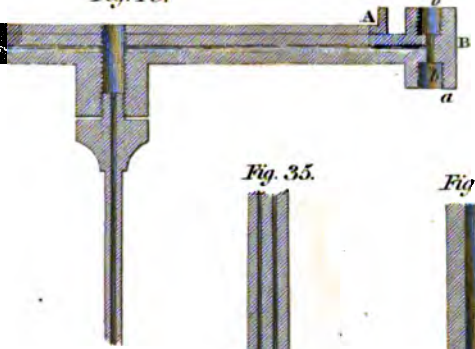


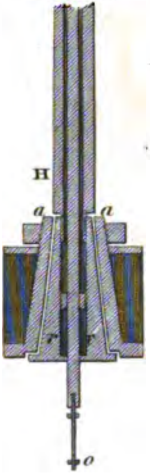
PNEUMATICS. VOL. III. PLATE XI.

Fig. 40.



ig. 38.

Fig. 35.



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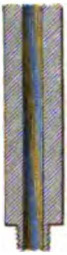
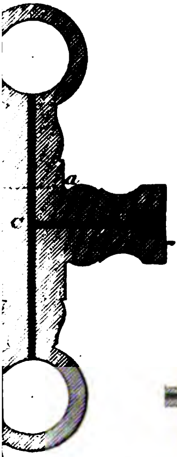


. 41



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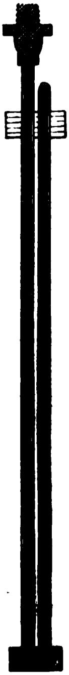

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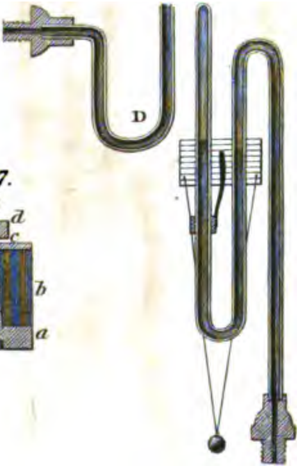

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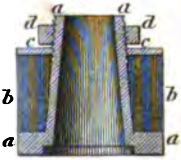

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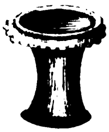
PNEUMATICS. *VOL. III. PLATE XII.*

Fig. 49.



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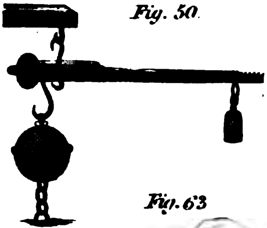

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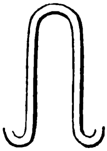

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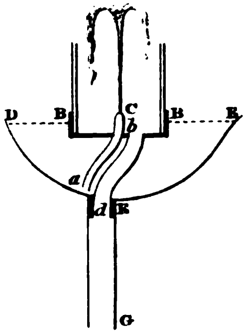

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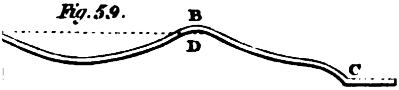

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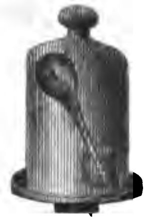
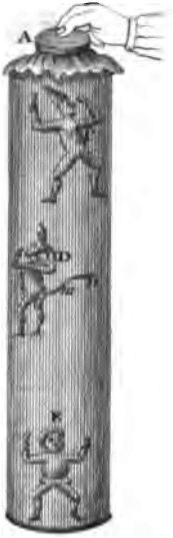


Fig. 63



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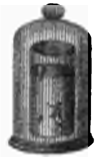

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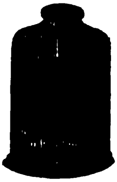
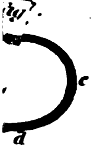


PNEUMATICS. VOL. III. PLATE XIII.



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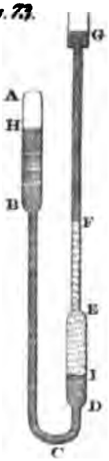

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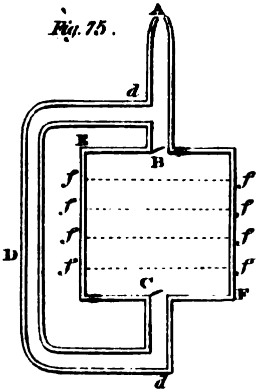

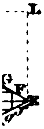

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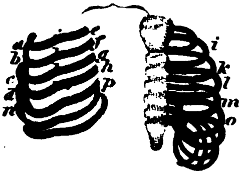

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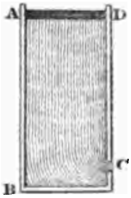

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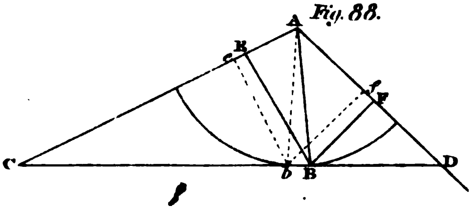

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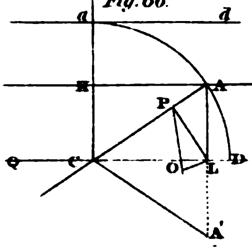

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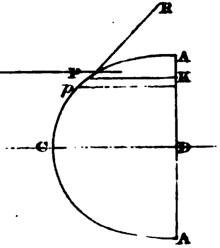
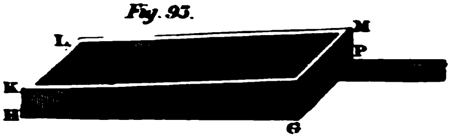


EUMATICS. VOL. III. PLATE XIV.

Fig. 93.



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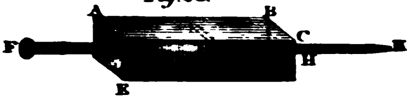


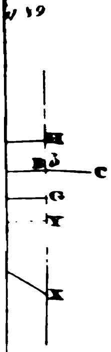
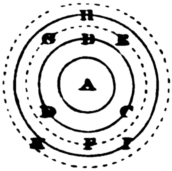


Fig. 94.

Fig. 91.



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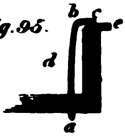

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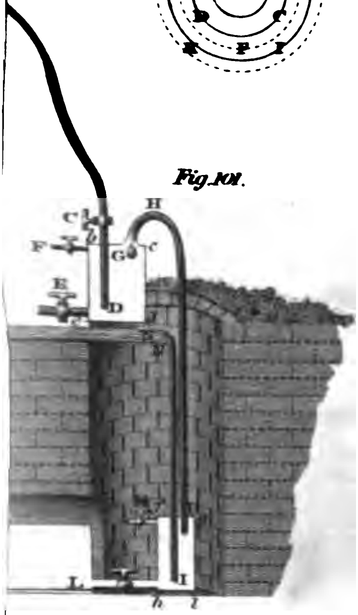

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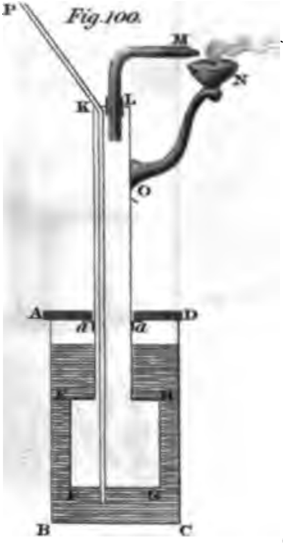

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